









ORIGINS AND DIFFUSIONS IN ARCHAEOLOGICAL THEORY

Colin Renfrew

Interviewed by Richard Cándida Smith

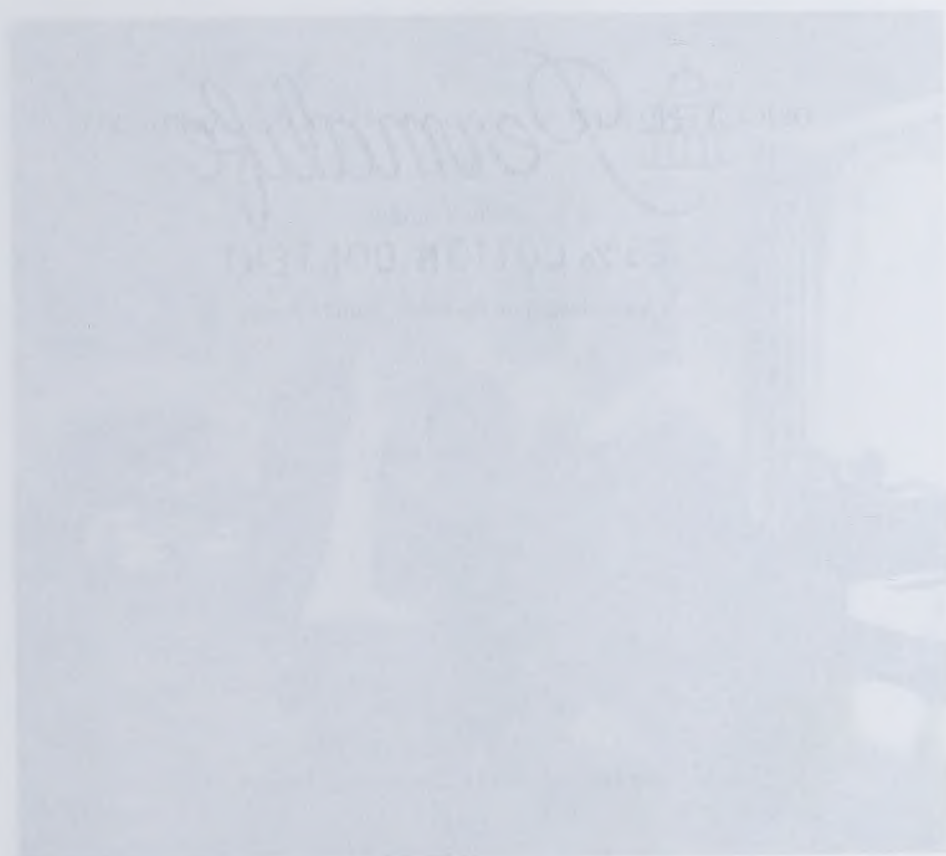
Art History Oral Documentation Project

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None.

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Frontispiece: Colin and Jane Renfrew. Photograph by Bill Ray, courtesy of Colin Renfrew.



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SESSION TWO: 16 MAY, 1996 (255 minutes)

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DEPARTMENT OF CHEMISTRY
530 SOUTH EAST ASIAN AVENUE
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TO: _____
FROM: _____
SUBJECT: _____

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Richard Cándida Smith, Associate Professor of History and Director of the Program in American Culture at the University of Michigan, interviewed Colin Renfrew in the Master's Lodge, Jesus College, Cambridge, England. A total of 13.25 hours were recorded. The transcript was edited by Katherine P. Smith.

Note: In his review of the transcript, Professor Renfrew added a few comments to supplement information or to clarify his remarks; these are all included within parentheses to distinguish them from the editor's insertions, which are bracketed.



CURRICULUM VITAE

Colin Renfrew
(Lord Renfrew of Kaimsthorn)

Present Positions: Disney Professor of Archaeology, University of Cambridge;
Master of Jesus College, Cambridge; Director, McDonald Institute for Archaeological
Research

Born: July 25, 1937, Stockton-on-Tees, England; married Jane Ewbank in 1965;
three children.

Education:

St. Albans School and St. John's College, Cambridge

B.A. 1962: Natural Sciences Tripos, Part I: Archaeology and Anthropology Tripos,
Part II (First class honours)

British School of Archaeology at Athens, 1962-63, School Student

Ph.D. 1965, Cambridge University ("Neolithic and Bronze Age Cultures of the
Cyclades and Their External Relations")

Appointments held:

1956-58	Flying Officer, Royal Air Force (National Service)
1965-72	Lecturer/Senior Lecturer and Reader in Prehistory and Archaeology, University of Sheffield
1965-68	Research Fellow, St. John's College, Cambridge
1966	Bulgarian Government Scholarship
1967	Visiting Lecturer, University of California at Los Angeles
1972-81	Professor of Archaeology/Head of Dept., University of Southampton
1981-	Disney Professor of Archaeology, University of Cambridge
1981-86	Fellow of St. John's College, Cambridge
1986-	Master of Jesus College, Cambridge
1990-	Dorector, McDonald Institute for Archaeological Research

CHAPTER 1

The first part of the book discusses the importance of the study of the history of the United States. It begins with a discussion of the early years of the nation, from the time of the first settlers to the end of the Civil War. The author then discusses the period of Reconstruction and the subsequent years of the late 19th and early 20th centuries. The book then discusses the period of the 1920s and 1930s, and the period of the 1940s and 1950s. The book concludes with a discussion of the period of the 1960s and 1970s, and the period of the 1980s and 1990s.

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University Administration:

1968-72	Faculty Board, Faculty of Arts, University of Sheffield
1972-81	Faculty Board, Faculty of Arts, University of Southampton
1980-81	Acting Dean, Faculty of Arts
1972-81	University Senate, University of Southampton
1976-79	Chairman, Day Nursery Management Committee, Southampton
1980-81	Chairman, John Hansard Gallery Committee, Southampton
1981-	Faculty Board, Faculty of Archaeology and Anthropology, Cambridge
1983-84	Council, St. John's College, Cambridge
1984	General Board, University of Cambridge
1985-89	Fitzwilliam Museum Syndicate

Memberships, Activities, etc:

1968	Elected Fellow, Society of Antiquaries of London
1970	Elected Fellow, Society of Antiquaries of Scotland
1974-84	Member, Ancient Monuments Board for England
1974-	Trustee, Antiquity Trust
1974-81	Chairman, Hampshire Archaeological Committee
1975-81	Member of Council, Salisbury Museum
1975-81	Member, Wessex Archaeological Committee
1976-79	Member, Archaeological Area Advisory Committee for Wessex
1976-85	Member, Royal Commission on Historical Monuments (England)
1979-83	Member of Science-Based Archaeological Committee, SERC
1979	Awarded Rivers Memorial Medal of Royal Anthropological Institute
1979-	Member, Conseil Permanent of the Congr�s International des Sciences Pr�-et-Protohistoriques
1979-83	Vice-President, Prehistoric Society
1979-82	Vice-President, Council of British Archaeology
1982-84	Vice-President, Royal Anthropological Institute
1980-84	Member, Management Committee, British School of Archaeology at Athens
1980	Elected Fellow, British Academy
1983-86	Member, Historic Buildings and Monuments Commission for England
1983-	Member, Ancient Monuments Advisory Committee (HBMC)
1983-86	Chairman, Science and Conservation Panel (HBMC)
1984-92	Chairman of the Governors, The Leys School, Cambridge
1985	Honorary Member, Society for Cycladic Studies (Athens)
1988-91	Chairman, Forum for the Coordination of Funding in Archaeology



- 1990 Litt.D (hc), University of Sheffield
- 1991 Chairman of the National Curriculum Working Party on Art
- 1991– Trustee, British Museum

- 1991 Dr. (*honoris causa*), University of Athens; Honorary Member of the Archaeological Society of Athens
- 1991 Awarded Huxley Medal of the Royal Anthropological Institute
- 1991 Honorary Member, The Prehistoric Society
- 1993 Member, European Committee of the Human Genome Diversity Project
- 1993 Member, Foreign Schools Advisory Committee of the British Academy

Archaeological Excavations:

- 1964–65 Saliagos, Antiparos, Greece (Co-director)
- 1968–70 Sitagroi, East Macedonia, Greece
- 1972–74 Quanterness, Orkney (with Maes Howe and the Ring of Brodgar)
- 1974–77 Phylakopi, Melos, Greece
- 1987–91 Markiani, Amorgos, and Dhaskaleio Kavos, Keros (Co-director)

Publications (partial listing):

Books:

- 1968 (With J. D. Evans). *Excavations at Saliagos near Antiparos*. (British School of Archaeology, Supplementary Volume No. 2). London, Thames and Hudson.
- 1972 *The Emergence of Civilisation: The Cyclades and the Aegean in the Third Millennium B.C.* London, Methuen.
- 1973 (Editor) *The Explanation of Culture Change: Models in Prehistory*. London, Duckworth.
- 1973 *Before Civilisation: The Radiocarbon Revolution and Prehistoric Europe*. London, Jonathan Cape (1973, New York, Knopf; 1976, London, Penguin; 1979, New York, Cambridge University Press; 1979, Tokyo, Iwanami Gendai Sensho; 1984, Paris, Flammarion; 1986, Madrid, Istmo; 1987, Rome, La Terza).
- 1977 (Editor) *British Prehistory, A New Outline*. London, Duckworth.



- 1979 *Problems in European Prehistory*. Edinburgh University Press.
- 1979 (Editor with K.L. Cooke) *Transformations: Mathematical Approaches to Culture Change*. New York, Academic Press.
- 1979 *Investigations in Orkney*. (Research Reports of the Society of Antiquaries of London, no. 38). London, Thames and Hudson.
- 1981 (Editor with J.D. Evans and B.W. Cunliffe) *Antiquity and Man: Essays in Honour of Glyn Daniel*. London, Thames and Hudson.
- 1982 (Editor with J.M. Wagstaff) *An Island Polity: The Archaeology of Exploitation in Melos*. Cambridge University Press.
- 1982 (Editor with S. Shennan) *Ranking, Resource and Exchange*. Cambridge University Press.
- 1982 (Editor with M.J. Rowlands and B.A. Segraves) *Theory and Explanation in Archaeology*. New York, Academic Press.
- 1983 (Editor) *The Megalithic Monuments of Western Europe*. London, Thames and Hudson.
- 1984 *Approaches to Social Archaeology*. Edinburgh University Press and Harvard University Press.
- 1985 *The Archaeology of Cult: The Sanctuary at Phylakopi*. London, British School of Archaeology at Athens.
- 1986 (Editor with M. Gimbutas and E.S. Elster) *Excavations at Sitagroi: A Prehistoric Village in North East Greece*, Vol. 1, Institute of Archaeology, University of California at Los Angeles.
- 1986 (Editor with J.F. Cherry) *Peer Polity Interaction and Socio-Political Change*. Cambridge University Press.
- 1987 *Archaeology and Language: The Puzzle of Indo-European Origins*. London, Jonathan Cape, 1987; 1989, Rome, La Terza; 1990, Paris, Flammarion; 1990, Barcelona, Editorial Critica; 1990, Stockholm, Symposion; 1990, New York, Cambridge University Press; 1992, Oslo, Pax.
- 1988 (with Glyn Daniel) *The Idea of Prehistory*. Edinburgh University Press.
- 1991 (with Paul Bahn) *Archaeology: Theories, Methods, and Practice*. London, Thames and Hudson.
- 1991 *The Cycladic Spirit*. London, Thames and Hudson; New York, Abrams; Athens, Goulandris Museum.
- 1994 (Editor with E. Zubrow) *The Ancient Mind*. Cambridge University Press.

Articles:

- 1964 "Crete and the Cyclades Before Rhadamanthus," *Kretika Chronika* 18, 107-141.
- 1964 "The Characterisation of Obsidian and its Application to the Mediterranean

- Region," *Proceedings of the Prehistoric Society* 30, 111–133 (with J.R. Cann).
- 1965 "Obsidian in the Aegean," *Annual of the British School of Archaeology at Athens* 60, 225–247 (with J.R. Cann and J.E. Dixon).
- 1966 "Obsidian and Early Cultural Contact in the Near East," *Proceedings of the Prehistoric Society* 34, 319–331 (with J.E. Dixon and J.R. Cann).
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SESSION ONE: 15 MAY, 1996

[Tape I, Side One]

SMITH: The question we usually start with is very simple: When and where were you born?

RENFREW: I was born on July 25, 1937, in Stockton-on-Tees, which is a town in the north of England. But we didn't live there for very long, so I have no recollections of that at all.

SMITH: Was your family from the north?

RENFREW: Yes, but not from the north of England. Both my parents were Scottish; my father [Archibald Renfrew] was from Glasgow and my mother [Lena Savage] was from a coastal town in Ayrshire, not very far from Glasgow, namely Ardrossan. After my father finished university in Glasgow, he joined a large firm, Imperial Chemical Industries [ICI], and we were then posted to Stockton, living in a village called Eaglescliffe.

SMITH: Did you move around during your childhood?

RENFREW: Not very extensively. That was the first place we lived. Then, because of the risk of bombing, we were actually evacuated from there to a more rural locality, not so far away, called Barnard Castle, of which again I have very hazy memories. I must have been two or three at the time. Then we moved down to Welwyn Garden City, which is about thirty miles from London, and we lived there

throughout my childhood. We finally left there in 1965.

SMITH: During that time was your father working in London?

RENFREW: No, he was working in Welwyn Garden City at ICI, in the plastics division, of which he became a director.

SMITH: He was a chemist then?

RENFREW: He took his university degree in chemistry, and so I think his early work was in that direction, concerned with plastics, including polythene, but he became a sales director, so that he wasn't engaged in chemical research for very long. Initially he was involved in production research, and then he moved on to the sales side.

SMITH: Did your mother work?

RENFREW: No, she worked at home, as it were; she didn't have an academic calling. She did later become interested in archaeology, and she took some adult education classes, but she wasn't an academic person, essentially.

SMITH: Did she have a university education?

RENFREW: No, she didn't.

SMITH: I assume your father must have.

RENFREW: He was at the University of Glasgow and did a first degree there, but he didn't continue to do a research degree, which I think few people did, unless they intended to have a research career at that time.

SMITH: What was the level of culture in the household? Were your parents



interested in the arts and music and contemporary literature?

RENFREW: Yes. My father had a very lively mind. He read very widely and enjoyed traveling very much. In his younger days he had clearly been interested in contemporary poetry, and perhaps prose writing also to some extent, so he read a wide range of works, but I think probably more for their content for instance than for their style. He always took the chance to see a good art gallery, though I think he was more interested in old masters than in contemporary work. He had perhaps a skepticism towards some contemporary work. He would enjoy going to the theater, but perhaps more for entertainment. He was less interested in music, it's fair to say.

My mother also had a lively interest in these things. She didn't read quite so widely, but nonetheless would keep up with things. She too would enjoy going to the theater from time to time. She would sometimes go to London to see the galleries with a friend who was also very interested in contemporary art. So I wouldn't say they were in a great intellectual milieu; their friends were mainly from ICI, but they had lively interests, especially my father.

SMITH: Was your family religious?

RENFREW: No, I wouldn't say so. My father was skeptical about many religious propositions, and my mother was not very actively interested, so they certainly weren't regular churchgoers. They might go to a Christmas service or something, but that doesn't necessarily make one deeply religious.



SMITH: When they did go was it Church of Scotland, or Church of England?

RENFREW: I was baptized into the Church of England, and that I think was mainly because if you were going to be baptized and you were living in England, you would be baptized in the Church of England, unless you made special arrangements, because that's what the church was. So that fits with what I was saying.

SMITH: Did you have much of a Scottish identity as you were growing up?

RENFREW: Well, to some extent. My parents would go to Burns' nights, for instance, which is what many expatriate Scots would do in England, but the Scottish element came through mainly from holidays. We would usually go up to Scotland for a summer holiday, and sometimes we stayed with my [paternal] grandmother and my father's sister, Aunt Florrie; they had a house in Glasgow, the district of Giffnock, and that was pleasant in its own way. Then sometimes we would go and stay with my mother's mother in Ardrossan. I never met either grandfather, but I knew the grandmothers quite well.

My [maternal] grandmother lived by the sea, indeed in the house where my mother had been brought up, in Ardrossan. My mother was one of a large family, so I had many aunts, uncles, and cousins on that side, so that was good fun. We often used to go across to the island of Arran, in the Firth of Clyde, as my mother had done in her childhood. It was a very pleasant place for holidays, good climbing and walking and so on. My parents had friends from their childhood who would



sometimes go, and we'd make a party of people. So we had very convivial occasions there—this was during the war years—and that's where I became well-acquainted with my relatives, all of whom of course are Scottish, and nearly all of whom did live, and indeed continue to live, in Scotland. So there's an element of Scottish identity there, but I've never really considered myself profoundly Scottish. I've never lived in Scotland; it's always been for holidays and family visits.

SMITH: On these excursions, did you experience an introduction to British archaeology? Would you go and play around fortifications?

RENFREW: Well, not exactly that. I've always been attracted by old buildings and found them somewhat romantic, so in Scotland it would be natural to go and see a ruined castle at Ardrossan, which is quite a romantic spot, and there are comparable ruined castles at Loch Ranza, on the island of Arran. Certainly it was there that I saw one of my first prehistoric monuments—the stone, megalithic tombs, those Giant Graves, but it wasn't a matter of huge fascination. I do remember, when I was really quite young, four or five, my father used to take me cycling around the countryside, and we would often visit parish churches. As you know, in England there are marvelous medieval churches as you go around the countryside, with brasses and to some extent stained glass windows. We used to enjoy doing that; it involved a Saturday out. So these outings involved, in a way, categories of monuments; they were medieval monuments mainly, and I've always found them beautiful and



interesting. That was clearly my father's view, or we wouldn't have been doing that at that time.

SMITH: As a child, or teenager, did you make any trips to the continent, or overseas?

RENFREW: Yes, I very well remember our first trip overseas. The war ended of course in 1945, and travel overseas was quite difficult, partly for financial reasons. We made our first trip in 1949, when I was twelve, and that was a very memorable trip. Two or three years earlier, my father had got a car; it was an open car, a very nice red MG with four seats, and he, my mother and I went across on the boat to France, and we went to Caen, which was very badly ruined by the war. I don't think the Bayeux tapestry was on show then. Then we went down through France to Italy, and it was on that visit that we discovered the Etruscans for ourselves, and that was completely by accident. Something had gone wrong with the car and we had been in a garage somewhere north of Rome, and another English traveler had said, "Oh, you ought to go and see this extraordinary sight," which was the site of Cerveteri. There we found tombs on all sides; it was a place where a lot of tombs had been opened over the decades. We investigated those and found quantities of pottery, which certainly I was collecting up, as a schoolboy would. That was the beginning of quite a serious interest in the Etruscans, which I've subsequently maintained.

About ten years later, when I was at university, I took Italian courses. I did a



course in Siena, and the following year at the University of Perugia, where they have a university for foreigners and they do holiday courses, and that was very good fun. Though later in my own work I became diverted to Greece, it would have been very easy to have done Etruscan archaeology, which I still find a very interesting subject. So that trip was very interesting. We saw some of the major sights in Florence, certainly in Rome, the Colosseum, Sistine Chapel, all these things, and down to Naples to see Pompeii and Herculaneum, which I find very spectacular. So already, though that was only fun, we were looking at archaeological sites and historic sites and finding them very much of interest, as many tourists do.

SMITH: What was there in particular about the Etruscan monuments that appealed to you? On a trip like that, your fantasy could have been sparked by any of several dozen things.

RENFREW: That's right. I think probably there were two or three factors. One was a sense, really, of discovery. Obviously the tombs had been entered before, but there were large amounts of pottery, some of it decorated, which had obviously not been looked at very seriously. Some of these tombs are really quite mysterious and elaborate; you go in with a torch and you creep around. So that was one factor. Another is that Etruscan art does have particular interest; it has a certain strangeness. The earlier Etruscan art in the archaic style differs rather from the Greek style, and then it goes on in the high period of Etruscan art with rather mannered qualities which

1. The first part of the document discusses the importance of maintaining accurate records of all transactions and activities. It emphasizes the need for transparency and accountability in financial reporting.

2. The second part of the document outlines the various methods and techniques used to collect and analyze data. It includes a detailed description of the experimental procedures and the statistical analysis performed.

3. The third part of the document presents the results of the study. It includes a series of tables and graphs that illustrate the findings of the research. The data shows a clear trend of increasing activity over time.

4. The fourth part of the document discusses the implications of the findings. It suggests that the results have significant implications for the field of research and may lead to further developments in the future.

5. The fifth part of the document concludes the study. It summarizes the main findings and provides a final statement on the importance of the research.

differ quite strongly from the classicism of the Greeks, though I perhaps wouldn't have put it in quite that way at the time. That is indeed what has attracted many people to Etruscan art over the years. D. H. Lawrence, when he wrote *Etruscan Places*, was making quite similar comments, which are actually rather obvious when one's looked at Etruscan art and compared it with Greek art. So that's a second factor. Then the third factor is that there was reckoned to be something of a mystery about the Etruscans. To some extent there still is. Their language is still not very well understood; it's a language which is generally regarded as not being an Indo-European language, which most of the languages of Europe are. It was at the Università per Stranieri that somebody, perhaps the art historian Enzo Carli, made the comment that "the things which you see are beautiful, the things which you know are more beautiful, but the things which you don't know are most beautiful of all." And there's something in that, there's nothing like a good mystery for exciting the imagination.

SMITH: You went to St. Albans School. Where was that located?

RENFREW: That is in the city of St. Albans, which is a cathedral city. St. Alban was the first martyr in England, and the cathedral goes back to the Saxon period, although it's mainly a very fine Norman construction. It has the longest nave, I think I'm right in saying, of any cathedral in England. The school is near the cathedral, and part of the school is the abbey gateway, which is a building from the fourteenth century. It's

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about seven or eight miles from Welwyn Garden City, where I went to Sherrardswood School, which is both a primary and secondary school. It was my mother, actually, who took the firm view that Sherrardswood School wasn't very challenging. St. Albans School certainly ranked as a public school within the British system. The headmaster is a member of the Headmasters' Conference, and although it wasn't principally a boarding school, nor was I a boarder, it nonetheless fell in that category.

My mother entered me for what was at that time the "eleven plus" examination. If you passed it, you would get free schooling at the equivalent of a grammar school, as they used to be called, and a free bus pass. My father could easily have afforded to pay and I think would have if prompted to do so by my mother, but I got the scholarship and so boarded the bus daily with other contemporaries. St. Albans was a good school with a lot of very good masters. It was a boys school. Sherrardswood School was a coeducational school, but at that time nearly all grammar schools or the equivalent were single sex schools in England. St. Albans did prove a very good place, with lots of interesting people, so it was a good experience.

SMITH: Who was there that was interesting?

RENFREW: Well, the headmaster himself, not that I saw that much of him, was a lively man. But I think one thinks more of the teachers one had, or indeed the friends one had, and I've kept a number of friends. One exact contemporary in my class was



Joe [J. R.] Cann, who went on ahead of me—because I did National Service first—to St. John's College in Cambridge. He did mineralogy and petrology and then became a professor in geology. Our preexisting friendship made it very easy to do a joint paper together, which was the first paper I did about the obsidian trade. I knew Joe from the age of eleven or perhaps slightly earlier, and there were other people in that class that I've kept in touch with. But it's mainly the teachers I think that one remembers.

Geoffrey Pryke, the physics teacher in the lower part of the school, was a very lively interesting man, who also had a great keenness for theatricals. I got involved in a number of theatrical performances with him which were interesting. Geoffrey Pryke also used to organize concert parties to London, to the Albert Hall or places like that, to hear orchestral concerts with a group of pupils of the school. Mr. McLellan was a really first-class English teacher, who helped heighten one's interest in language and awareness of language. Another master I remember with pleasure was the physics master, Mr. Marshall, who was very clear and very willing to argue things out, so that he allowed one to develop a skepticism. If you didn't understand something you could say, "That can't be right," and he would be willing to argue it through and be willing to be proved wrong—not on many occasions, but he did have that quality. I think he was a very good teacher.

There were others who were good. One very nice man was John Coles. He was the Latin master, and though I didn't particularly enjoy his classes, when I began



to be interested in the possibility of going on an archaeological excavation, he was the person who managed to set that up. He knew the curator of the local Roman museum, Mrs. Audrey Richards, because St. Albans is near the major Roman site of Verulamium, and so he spoke to her. She had a very good friend who was directing the excavations in Canterbury, Sheppard Frere, who later became professor of Roman archaeology, and indeed Britain's leading Romanist. I suppose I was thirteen or fourteen at the time, so I went off as a schoolboy to these excavations. So St. Albans was a place that encouraged interests in a very positive way. There are other masters I could mention of whom I have very positive recollection.

SMITH: As a teenager did you continue participating in archaeological excavations?

RENFREW: Yes, I liked the setup in Canterbury very much. Sheppard Frere was a nice, encouraging man. He had one or two assistants who were I think a little amused at having somebody really much younger than usual, but obviously Mr. Coles had spoken well of me and said that I wasn't going to be any great trouble, and I don't particularly believe I was. I may not have been a great deal of use to start with, but I enjoyed excavating, particularly on Romano-British sites, where you find quite a lot of pottery, and from time to time you find a coin. Frere was a first-class archaeologist in the good stratigraphic tradition of British archaeology, of which Sir Mortimer Wheeler is a very good example, and so for me that was really good insight into just the basics of recovering information from the ground. I went back to Canterbury for



four or five years, usually Easter and summer, and one year I persuaded Joe Cann, whom I mentioned, to come with me; it was a really pleasant experience.

SMITH: It sounds like by the time you arrive at St. John's you already knew that you would do reading in archaeology.

RENFREW: Well, you might think so, but when I was at school, at the age of about thirteen or fourteen, you had to make the decision whether you were going to do science A-levels, which were the relevant examinations, or arts A-levels, following the O-levels. The O-levels were taken at the age of sixteen, but you would decide before that which way you were likely to go. So the O-levels are the intermediate level examinations, and the A-levels are more or less the final school examinations. I decided that I seemed to be learning more with the science subjects, and I was doing quite well in them. I remember the headmaster actually saying that he wondered if I shouldn't be staying on the arts side. He wrote a very long report to my parents when I finally decided, saying, "Qualis artifex perdio" at the end of the report when I finally went towards the sciences, perhaps rather against his advice; it seemed a more productive way to think of getting a career.

So when I went up to St. John's it was to read natural sciences, using initially the A-levels, which were maths, physics, and chemistry. The tripos in Cambridge is unusually flexible in that it always has two parts, and it's quite common for people to change part way through. I did part one in natural sciences, which was a two-year



course at that time, and I enjoyed that, but I was beginning to ask myself what I would do next. It had become clear to me I wasn't going to be a research physicist or research chemist or biochemist, and I had begun to think very seriously about doing archaeology for part two, perhaps as a career. Obviously I had good experience with excavation, but I hadn't done very much serious reading in archaeology until I started doing it at university after two years.

SMITH: Now, you said before you went to university you did your National Service?

RENFREW: That's right.

SMITH: As I recall you spent two years in the air force?

RENFREW: That's right, yes. That turned out to be fun, more than I had expected. I've never been terrifically good at games and not hugely physical in that sense, so I thought that National Service might be rather a trial, but because I had been in the air force section of the corps at school, where I learnt to pilot a light aircraft, I was given direct access to officer training, which is what I did. So in that way I missed the first eight weeks' or three months' "square bashing," as it's called, just drill, very basic induction training, and I went straight to the officer training on the Isle of Man, which is a beautiful place to be. That was deliberately quite tough. They put you through your paces and so on, but you were in with a lot of other people and had plenty of laughs, and it was quite fun in various ways.

Then I was very fortunate to get a posting to Germany. I think it might have



been rather boring to have been at an air force station in England, so I got a posting to the Royal Air Force Wunstorf, which was near Hanover, and that was an opportunity to see something of Germany. I was able to buy a series of cars, all of them rather unsuccessful as cars but successful as an experience. I also learnt some German. I took some lessons privately and then was able to get sent on a course which the air force paid for, which was good enough to get just O-Level German. Not a very high standard, but I had plenty of conversational experience, so that my spoken German was perhaps better than O-Level standard.

I was able to travel down to Vienna and go across to Berlin. I spent some time in Munich, so it was in many ways a very pleasant two years. Towards the end of that time my flight commander asked would I care to stay in the air force; he thought it would work out, and although I had a university place, it didn't seem a ridiculous suggestion. I think if I had been asked earlier, I would have said absolutely not, but in fact it didn't seem a weird suggestion. I liked the people and had had a very good time. What was nice about it above all was that one was with at that time—we're talking about 1956–58—a lot of older people who had been in the air force during the war, so I was mixing with a group of people who had seen a lot of different experiences, and that was very interesting.

SMITH: But you decided to go to university. What governed your choice of the school you went to?



RENFREW: When you say "the school," do you mean the university or the college?

SMITH: The university, the college, I suppose both.

RENFREW: Right. It was assumed at St. Albans School that if you were bright enough you would go to Oxford or Cambridge, and if you were bright enough again you would get a scholarship to Oxford or Cambridge. I think in retrospect all that was a bit of a . . . I don't want to say a con, but it really meant more to the school than to the pupil, because the school announced the number of scholarships or exhibitions to Oxford or Cambridge, and that reflected well on the school. The truth is, a scholarship or exhibition didn't bring any significant benefits to somebody going up to Oxford; it wasn't any longer of any financial significance. If you were admitted for entry to any British university and you were a British citizen, you would get a state scholarship, which would not only pay your fees but would also pay your living costs really quite adequately. Unfortunately, in recent years that is no longer the case. So the benefit was in a way partly to the school, but it also meant that you stayed on longer. You would stay on a seventh term in the sixth form; in other words, you would start in September and do a term before going up in December to Cambridge for the exams, so in a way you lost a year, or you spent a year doing other things, because you stayed on at school for an extra term just polishing up skills which you didn't really need to sit examinations, then that gave you two more terms to while away.



Cambridge seemed a natural choice, I think mainly because the headmaster at St. Albans was a Cambridge man, and so the question was, were you going to go to Oxford or Cambridge? Well, Cambridge was more natural. It was also geographically closer. Why St. John's College? I think it's very difficult in Cambridge to know which college to choose, and I thought maybe Trinity was too large, which may well be the case, and St. John's was the next largest. It's also a very beautiful college; it was a good decision, and it proved a very nice place to be.

SMITH: You started out doing natural sciences; was this for your first year, or was it two years?

RENFREW: It's two years. As I was saying, the tripos course in natural sciences at that time was a two-year part one, followed by a one-year part two, so it was natural to do the part one examinations and pass those, and that technically gave you the qualification for the degree, so that if you'd wanted to, you could just do something else. You'd have to do a third year in university, but it's enough examination qualifications for a degree. It was at that point I decided to turn to archaeology. It would have been possible to do part one archaeology in one year, but that wouldn't get you far. Or one could do part two archaeology in one year, but the part two archaeology course is normally a two-year course, and in addition you wouldn't have the benefit of having done part one, so to do part two archaeology in one year would be rather ambitious; you couldn't expect to do very well. So it was agreed that I



should do two years of part two archaeology, which meant doing a fourth year at university, whereas the B.A. course was and in many ways is still normally a three-year course. But that wasn't difficult to organize, and so that's what happened.

SMITH: Before we go on to the archaeology, I'd like to just spend a little bit of time on the natural sciences. Who was your director of studies and what was the most interesting aspect of what you were studying?

RENFREW: Right. I had a tutor in St. John's who was Mr. Welford, but his role was generally welfare, so you'd go and have a glass of sherry with your tutor at the beginning of term and the end of term. He was a very nice man, rather avuncular, so that was pleasant, and if one had problems (and I didn't have any terrifically grave problems) or there were any issues that needed discussing, he would be very happy to give advice or help. I think he organized the directors of studies in the different subjects. I remember Dr. K. G. Budden, who was a physicist, but one has separate supervisions in all the different subjects, and in those days you had to build up three and a half whole subjects, and I seem to remember whole subject maths, whole subject physics, half subject organic chemistry, half subject biochemistry, maybe it was additional maths, which was a half subject, and the history and philosophy of science, which was a half subject, which I actually found the most interesting. Biochemistry was also particularly interesting. It was a time of great change, when biochemistry was really opening out in Cambridge, it was just shortly after Francis

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5. The fifth part of the document concludes the study. It summarizes the main findings and provides a final statement on the importance of the research.

Crick and James Watson had done their work there, and there was quite an exciting atmosphere in the department. Because they were quite large lecture groups, one was lectured to by really very well informed active researchers, which was nice.

I found the maths quite hard going. I think it may be that if I had really been an absolutely born mathematician and had had no problems at all, I might well have thought of doing research in fundamental physics; certainly I found that very interesting, but my maths never really came easily enough to have instant intuition into quantum theory or relativity theory. I should have mentioned that before I went up to Cambridge I spent some time in France and went to lectures in a wide range of subjects at the Sorbonne. I remember Robert Oppenheimer giving a course of lectures on the current state of fundamental physics, which was very intelligible and very interesting. It was made doubly interesting by the fact that Oppenheimer himself was such a high profile figure; people were fighting to get into the lectures. He had been so much in the news with all his legal problems and so on. I also remember very clearly Louis de Broglie, who had won the Nobel Prize in the 1920s for basic research, sitting and listening to some of the higher physics, which I wasn't understanding at all. Now, that was an aside in relation to your question of what was interesting, but I thought the history and philosophy of science was really a very interesting course.

SMITH: Who was teaching that?



RENFREW: [Gerd] Buchdahl was a very inspiring man who had a great sense of problem on the philosophical issues and communicated that. There were, and to some extent still are, problems in the philosophy of science. The exact status of theories and of models and what really leads you to believe what you do believe was debated, and of course Karl Popper's work was much followed. But it didn't provide all the answers, and indeed since that time there have been considerable developments in the philosophy of science. It was an era which has been criticized since as being rather positivist.

On the other hand, I very much remember being invited to read, as part of the course, the article by Carl G. Hempel and Paul Oppenheim which more or less asserted that all historical explanation is of the nature of law-like explanation. It was the article on which Hempel's subsequent books were based, and I remember seeing that it really didn't make sense; that that wasn't the way historical explanation worked or should work, but at the same time I was unable to show at what point the argument went wrong. I'm not sure anybody ever has shown at what point it went wrong. Subsequently there was a great swing away, as I'm sure you know, from that sort of approach even in the philosophy of science, and certainly more so in other fields. This was certainly very useful when one was looking at theory in archaeology, which was and to some extent is a rather ill-developed field. But to have this interesting discussion of the nature of the philosophy of science, having done science



at school and at university, was really very stimulating. Gerd Buchdahl, although he didn't write a great deal, had a wonderful sense of problem, and he managed to relive it during the class, so that one could really see what the problem was. Because I think he was genuinely unsure of what some of the answers were, when one was making comments they would seem to him interesting, and that was enormously stimulating. He was one of the best teachers I had at university. We had also an interesting teacher in the history side, Michael Hoskins. But Gerd Buchdahl, with his sense of problem, was enormously refreshing.

I wrote quite a lot of essays, and in my second year at university I was invited to give a paper to the university's History and Philosophy of Science Society, which I did, on the subject of simplicity. I have always mildly regretted that I didn't publish that paper as I was invited to do and might have done if I hadn't had all my footnotes in disorder; it would have been an enormous effort to go and recover the references. But the concept of simplicity, which of course has always been a key one in scientific theories, at that time had not been very well discussed, and it was very interesting to look into. It touches on issues like beauty and its appropriateness as a criterion in scientific theories. Obviously, theories have to explain the facts, but given that, what is the better explanation? Well, it's clearly the more simple explanation, but my paper took one into the literature and the view of mathematicians that good explanations are beautiful as well as being effective. So that was an interesting area of study to get



into, and it has remained so. And it was, as I say, particularly interesting to have already been acquainted with that work of Hempel and Oppenheim, which I suppose had been written about ten years earlier, long before it came up as a subject of discussion in archaeology, which was many years later.

SMITH: Aside from the broad range of social and socio-linguistic aspects that are present in your work, there's also a very daunting array of demographics, statistics, and econometric work. To what degree were you prepared for thinking mathematically through your natural science work?

RENFREW: Well, I'm sure that was one element. Just as significant I think was the attitude that I would not be put off by science because of a lack of understanding. I think anybody that works in science, if there are several fields they are looking into, must expect not only to be not well-informed but also sometimes not to understand the entire reasoning. I soon found that there were flights of mathematics that at that time were beyond me and probably would have remained beyond me, but that did not prevent one seeing that some things made good sense and were clearly good science. You don't have to understand every word in detail to see that it's good science, though to some extent you may have to rely on other people's work. Having developed a skeptical disposition already at school, I very soon came to expect that you can be shown the logic of something if it's there, but, equally, you can always go back and say, "Where is the logic?" So I think that was a useful experience.

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I use an array of techniques, that's true, but it became clear to me that you don't have to understand fully all the techniques that you use. It isn't sloppiness, or unscientific, for instance, to use statistics in collaboration with a statistician. I've made reference to catastrophe theory, which is a difficult field of mathematics, and I certainly couldn't reproduce the mathematics. I think a good basic training in science does allow one to see how to work in collaboration with scientists, whereas I think many people who haven't worked in science rather close their minds and just say, "I can't understand all that stuff, don't bother me with that." My school experience more than my university experience led me to see the merits of numeracy, so that some of the work I've done on trade, which I did understand fully, came out of good school experience in the sixth form: doing experiments and seeing how equations work, and in that sense having a genuine insight into what science is.

I'm often scandalized, to just broaden out for a moment, in contemporary society, how poor the grasp of scientific reality is. I think it's just breathtaking, the widespread ignorance of science and widespread unwillingness to exercise some discretion in the field of science. You get terrible canards in the press; for instance, the whole debate about whether the HIV virus is causative for AIDS was very badly handled in the British press just because people are not willing to ask what is good scientific argument.

SMITH: Of course '59 is the year that C. P. Snow publishes *The Two Cultures*, and it



became a major topic of conversation for a while.

RENFREW: That's right. Though I don't think I ever saw C. P. Snow. He was of course a fellow of Christ's College.

[Tape I, Side Two]

RENFREW: His book *The Masters* is about magisterial elections in Christ's College. But if you remember, C. P. Snow's thesis was then challenged by F. R. Leavis, who was this extraordinary critic in the field of English literature here in Cambridge. I went to some of his lectures. I was astounded by his really really acid, coruscating criticism of the people with whom he didn't agree. I've never heard anybody be so rude about anybody else as F. R. Leavis was about the people of whom he didn't approve. If you didn't know the people, and if it wasn't coming in your direction, which obviously it wasn't, it was just a pleasure to listen to somebody who could be so contrivedly malicious! So you're right, that debate was very active at that time, and although in some ways it's been judged since to be a slightly artificial debate, I don't actually think that it was.

The people who write book reviews in the press are very often people with total ignorance of how science works in the most general sense. You know about the present great controversy in Britain about mad cow disease and whether that has a link with Kreutzfeld-Jacob disease in humans. Well, a lot of the argument is just appalling, and the public reaction as to whether they buy beef or not, whether it



should be banned or not, is not really based on assessment of the evidence, even though in this area the evidence is quite difficult to assess.

SMITH: So you shifted into archaeology and anthropology, or was that a combined field in this case?

RENFREW: In Cambridge, the tripos is Archaeology and Anthropology, but unfortunately, really since its beginning in Cambridge, although it's one tripos, the two subjects are taught together only in part one, which is a one year course, and then you decide whichever you choose not to do. So if you are reading part two archaeology in Cambridge you get absolutely no teaching from the anthropology department.

Because I didn't do part one archaeology, I got hardly any anthropology. I tried to go to a few lectures, but it didn't work very well doing part two and part one at the same time. At that time I think a lot of teaching of anthropology was navel searching about the nature of kinship. I've always formed the view that kinship seemed to be an excessively well-developed field of research among Cambridge anthropologists and others, and I could never actually see what was so totally fascinating about how different communities configure their views of kin relations.

SMITH: Who did you read with in archaeology?

RENFREW: I was very fortunate that in St. John's College the director of studies in archaeology was a splendid person, Dr. Glyn Daniel. Long after I left Cambridge he became Disney Professor and head of the department. He was a delightful person,

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with great interest in other people—a noted *bon viveur*. He wrote a wonderful book called *The Hungry Archaeologist in France*, which is one of the most delightful guidebooks in existence; it deals with Lascaux and Carnac, and with the food of the Périgord, the seafood of Brittany, and so on. I already knew Glyn Daniel, just by sheer coincidence, in my first year in Cambridge. You are assigned a room, and my room was in E staircase, and his rooms were a couple of floors above, so I got to know him a little just to say hello to. I remember at the end of my first year I decided to have a wine party for friends, the first party I had organized. I invited Dr. Daniel, whom I didn't know well, to come to the party, which he did, very charmingly. So he was already good news before I began to take archaeology seriously.

When I began to think of changing to archaeology, halfway through my second year, I went to see my tutor first of all, Mr. Welford, no doubt over a glass of sherry, and he said, "Oh, well, you must talk to Dr. Daniel," so he spoke to Dr. Daniel, and I went to see him, and he was enormously enthusiastic and positive and said there would be absolutely no trouble. So everything was made very easy for me to change to archaeology. Glyn Daniel became my director of studies, so he organized the supervision program and gave me a good many supervisions himself. As you know, a supervision is where you go and you read your essay to the supervisor, sometimes with one or two others, sometimes on your own, so Glyn Daniel was certainly the person I saw most of.



He had the great gift, which many people have spoken of, of making all his supervisions amusing and interesting, anecdotal. He didn't always stick to the point: he was editor of the periodical *Antiquity*, and sometimes he would venture over what was happening in archaeology, the latest news, and sometimes eminent people would drop in. He would give parties from time to time if he had visitors. That was when I first met Sir Mortimer Wheeler, for instance. So Glyn Daniel was very good fun, and very encouraging, and he had particular interests, which came over in his lectures and in his supervisions. His own special field was the megalithic monuments of northwestern Europe. He had always been fascinated by them and hoped to communicate an enthusiasm. Obviously if one was interested in prehistory one would already be interested in sites like Stonehenge.

Given that I was doing the option in later prehistory, neolithic, bronze age, and iron age, not in the paleolithic period, I didn't see very much of Charles [C. B. M.] McBurney, the lecturer who was a specialist in the paleolithic. The anchorman, really, in the department, who did a lot of the lecturing and looked after the students very well was Dr. John Coles, who later became Professor John Coles in the university. He was of Canadian origin, and he lectured on the bronze age, and on other things. He was really a great help. The head of department was Professor Grahame Clark, whom I later got to know well as a research student, but he seemed at first, certainly by contrast with Glyn Daniel, a slightly remote figure, rather austere

THE
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in some ways intellectually, perhaps ultimately because he was of a more shy personality. He was in fact very welcoming, and he would have students round to his house. It wasn't quite the same warm atmosphere that Glyn created, but very courteous, very polite. He would invite us round for a meal sometimes, and we'd have a party for the external examiner at the end of the course. Grahame Clark was very genuinely hospitable, and I came to like him very well, but he wasn't so much fun as Glyn Daniel. So those were some of the main people in the department at that time.

SMITH: Was Dorothy [D. A. E.] Garrod still around at this time?

RENFREW: No, she had retired. She had been the Disney Professor of Archaeology, and she retired from that in 1952, before I appeared at Cambridge. Grahame Clark had been well-established as professor for six years. I only met Dorothy Garrod once, and that was through the kindness of Glyn. My research degree took three years, and during the previous year I had met Jane [M. Ewbank], who became my wife. I handed in my doctoral dissertation, and then we married and went off to our honeymoon in Paris, in 1965. It was there that we received an encouraging telegram from Glyn Daniel saying that I had a research fellowship in St. John's.

I'm halfway to answering your question. For a wedding gift, Glyn gave us a guidebook to Paris, but he also arranged that Dorothy Garrod, who had retired to



Paris, should invite us to lunch. She lived in the *pension* where Suzanne de St.-Mathurin who was another distinguished archaeologist, lived, and also the Abbé [Henri] Breuil's secretary—the Abbé Breuil was dead by then. So Jane and I went and had lunch with Dorothy Garrod and Suzanne de St.-Mathurin, which was a very pleasant occasion, but of course in retrospect is even nicer, because Glyn Daniel succeeded Grahame Clark as Disney Professor of Archaeology, Grahame Clark had succeeded Dorothy Garrod, and I succeeded Glyn Daniel. I am the tenth Disney Professor. Glyn was the ninth, Grahame was the eighth, Dorothy Garrod was the seventh. So it was very good to have the opportunity of meeting the seventh Disney Professor on that occasion.

SMITH: That is nice. Now, your dissertation was on the Cyclades, but you did not study classical archaeology?

RENFREW: That's right.

SMITH: Why was that? Or, perhaps what I'm inviting you to discuss is, what was the relationship of the archaeology program that you were in to the classical archaeology program, and how did things such as the Cyclades get divided up?

RENFREW: Right, that's a very fair question. My work in the Cyclades was on the prehistoric period. You're quite right. If you're dealing with the prehistory of Greece you can certainly debate whether that belongs in a department of prehistoric archaeology, or in the pre-Hellenic part of the department of classical archaeology.



Moreover, in Cambridge, the prehistory of Greece has, since the time of [A. J. B.] Wace, who lectured in Cambridge for a while, been taught in the classical archaeology department. In my day, Dr. Frank Stubbings, who was a member of the department of classical archaeology, was the lecturer. He was a man with classical formation, but a specialist in the Mycenaean period, and what we would still regard broadly as the prehistoric period. So I did go to his lectures as something extra while I was doing the undergraduate course in my final year, and then he indeed became my research supervisor at Glyn Daniel's suggestion, and very helpful he was, too.

I suppose the circumstance of choosing a research topic in Greece arose out of my first visit to Greece in 1961, when Mr. [R. J.] Rodden, who was one of Grahame Clark's research students, was conducting an excavation in a very interesting site called Nea Nikomedia, a tell mound. At that time it was thought to be about the earliest neolithic site in Greece. Rodden was beginning his excavations there, he'd done his prospecting work, and Grahame Clark was supporting him in various ways. Grahame Clark officially held the permit for the excavation, and Bob Rodden was recruiting site assistants to go out. He recruited them naturally from the department of archaeology, and so I and a few other people who later became professional archaeologists went out to Nea Nikomedia. It was my first visit to Greece, which I very much enjoyed.

On the excavation there was a splendid old gentleman, R. W. Hutchinson,

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always known by his nickname "Squire," who had retired years earlier from his lecturing job in Liverpool and had written the Penguin book on prehistoric Crete. He had been the curator at Knossos immediately after the war. He was going to Crete and asked would I care to go with him, so I said what fun that would be, as indeed it was, and we flew out. It was actually my first glimpse of the Cyclades. We flew out over the island of Melos to get to Crete. It was a wonderfully exciting experience to see these sites, and be shown them by somebody who knew them very well and who was a very kindly gentleman.

Squire Hutchinson knew absolutely everybody, so when we were at Knossos we were at once summoned in to have dinner with Sinclair Hood, who was the director of the British School and was excavating there. It was on that day that I met somebody who became one of my best friends, Peter Warren, who is now professor of classics and archaeology in Bristol. When we went down to Phaistos, Doro Levi, the distinguished Italian archaeologist was excavating there. At once he said Mr. Hutchinson must stay to dinner, and naturally I was with him, so I did too, and we got to meet the head of antiquities in Crete, Mr. [Stylianos] Alexiou. It was a wonderful introduction to Cretan archaeology.

I'm beginning to answer your question. This must have been at the end of my first year of archaeology, that's to say the summer of 1961. Then I went back to do my final year, and I began to wonder about doing research. In the National Museum



in Athens I had seen the very spectacular display of antiquities from the Cycladic islands, the bronze age, including the marble figurines, which seemed to me quite astonishingly beautiful. I had seen them before in the Louvre, which I came to know very well before going up to Cambridge. There was the most spectacular marble head, about eighteen inches high, of great simplicity. As you are rightly indicating, I have some replicas of them over there.

So I got curious about the Cyclades, and in my reading it became clear that not much more was known about them than had been known about forty years earlier, although a lot was written about Cycladic influences in Europe. It just struck me that this would be a very good subject for a research project. I mentioned this to Glyn Daniel, and he spoke to one or two people about it and advised me to write to Professor John [D.] Evans. He suggested that I should write to a number of distinguished archaeologists who might know something about it. Emboldened by his suggestion, I wrote to them, and they pretty well all wrote back and said, "What a good idea. We don't know much about this area, and it's well worth knowing about."

I was approaching the subject as a prehistorian, as did [V.] Gordon Childe, who had a chapter on the Cyclades in his book *The Dawn of European Civilisation*, which came out in 1925. It's been natural for many years to approach prehistoric Greece either as a prehistorian, which is quite a natural approach from the English educational system, whereas most of the United States scholars working in that field



would have a formation in classical archaeology. A good example is Tom [T. W.] Jacobsen, with whom I assisted Professor [John L.] Caskey, who ran the Kea excavations. We were working at the site of Kephala as site supervisors for him. It was natural for Jacobsen to go on to do the major excavations at the Franchthi Cave, and it's somehow natural that he would have been trained as a classical archaeologist initially, whereas I was trained as a prehistorian. That had advantages and disadvantages. It meant that I knew absolutely no Greek when I went out, so I learnt some modern Greek. I still don't know classical Greek to any serious extent. I always work with classical Greek in translation—the Loeb translations, or whatever. I do use the dictionary to think about vocabulary, but I am no classical scholar, so that was a disadvantage. On the other hand, one came with perhaps a fresher eye to some problems, and not burdened by the great tradition of art-historical scholarship like some of those whom you've interviewed—John Boardman, Cornelius Vermeule, or Dietrich von Bothmer. That's a rather different tradition.

SMITH: In Saliagos, and Melos, or at Sitagroi, did you actually need to have a little Greek in order to do what you needed to do?

RENFREW: I didn't need any classical Greek at all, but certainly in order to do an excavation you need to speak modern Greek. Fortunately that came in my first term as a research student in Cambridge, in 1962. When I started research I fortunately was able to get a copy of the great work, *Kykladika*, by Christos Tsountas [in



Archaiologike Ephemeris for 1898 and 1899] summarizing his excavations of the Cyclades. I found a Greek teacher, and we went through this, and that was one way to approach it, just to use some texts. It was rather formal Greek, being written in 1898 and 1899, but very soon I was trying to speak Greek. Indeed, my first experience, as I said, was being on an excavation, so already I had a very good introduction. The best way to start to speak a language is to start to speak it, and so I did, with the workmen on Bob Rodden's excavation, who were great fun. Rodden developed a wonderful relationship with his workmen, which I greatly admired. They were a young group, and he really treated them as friends. They were really colleagues and collaborators, and very good fun they were. We had parties and so on with them, so already I had some smattering of Greek, a few words, and a feeling for how it sounded.

When I went out to Athens, I took a few lessons and used that Teach Yourself series. I went out to Greece at Christmas, and then come March, it was time to go on some travels in the islands. I went on my own, or sometimes with a friend from the British School. There is no better way to learn a language than having to communicate in that language. It's even better to have no opportunity at all of speaking your own language. After a few months, given that I was working at learning the grammar, I was able to put a few sentences together, and then I had very pleasant experiences when I went to Antiparos, which is near Saliagos, and also



Naxos. I went up to a village museum in Apeiranthos, and there was a whole group of lads in their teens and twenties. At that time they really hadn't seen that many foreigners coming to stay—there was no hotel in the village—and they had nothing better to do than gossip and drink, so we used to party together, and I became quite fluent in colloquial Greek.

Two years later, in 1964, when I came for the first year of excavation at Saliagos, it was perfectly natural to communicate with the foreman and with the workmen in Greek. Being on an excavation and doing that is itself a wonderful learning experience, because you're speaking Greek nearly all the day. By 1965, the second year at Saliagos, my Greek was quite fluent. It was still simple, but it was fluent, and it's improved since. So although I have no classical Greek at all, I have lectured in Greek on occasions. I'm sure that to sophisticated listeners my vocabulary is a little basic, sometimes they may find rustic terms creeping in I think, but it's certainly quite good enough to discuss problems with workmen.

SMITH: As you set out to think about your research topic, was it a general assumption at the time that doing an excavation would be a primary part of your methodology?

RENFREW: No, absolutely not. No, the intention was to review what was known of the early bronze age of the Cyclades. When I started, nothing was known of the neolithic, and there was clearly abundant material to study in the museum that wasn't



understood. The chronology wasn't understood, the relations weren't understood, and so I started off doing that. One important thing to go and look at was the site of Phylakopi, which had produced at the time the only fundamental stratigraphy of early bronze age materials. So my intention was to go and work on the finds there, both in the National Museum in Athens and out in Melos, and when I did that, that was also an incentive to go and look at the obsidian sources, and that began to lead me in that direction.

I was looking at all the known sites, and I had one piece of luck. Professor Saul Weinberg, a very distinguished American archaeologist, put me onto some finds from the island of Mykonos, and I was able to study these. They were mainly obsidian artifacts, different from the others that were known, and it became clear to me that they were in fact neolithic. So this was the first sign of the neolithic. Then Professor Caskey began excavating the cemetery at Kephala, on Kea, which also was clearly late neolithic. Then I hit on this site of Saliagos, which had been mentioned very briefly by the Greek archaeologist Nikolaos Zapheiropoulos. I recognized that the pottery there was quite unlike anything that had been seen before, and was almost certainly neolithic, and so it seemed this would be an excellent site to excavate.

Fortunately, at the time, the British School was not overwhelmed by applications for excavation permits, so this [excavation] could be slotted in, and the management committee took the view that so junior a person ought to have a senior



person working with them, just as Bob Rodden at Nea Nikomedia had had Professor Clark in a senior capacity: he didn't actually do the dig. So I wrote to Professor John Evans at London to ask if he would consider joining me, and that proved a very pleasant collaboration. He was much senior to me but an extremely nice man, so we managed to have a collaboration on a very equal footing, and we've remained very good friends ever since. That was a very happy experience I think for both of us, and the site turned out to be very interesting.

Just to give you an anecdote: Evans hadn't seen the site until the excavation began, in 1964. It must have been in 1963 that I'd got photographs and material and so on. We arrived in Paros, off the steamer, and then had to take the caique to Antiparos, a caique being a small boat. Saliagos is a small island in the channel as you approach Antiparos. As we passed the island that evening, you could see it was very small, and Evans was clearly disappointed at the appearance of it and said, "Oh well, if there isn't enough to do there maybe we can find somewhere else to excavate." But when we went out there the next day, he could see there was a good depth of deposit, and we only excavated about one fifth or one sixth of the material that could have been excavated there, so it worked out very well. But your question is quite right. I think it's probably rather ambitious for any Ph.D. student, certainly in a well-traveled area like Greece, to have the expectation of doing an excavation as part of one's doctoral research, so that was a piece of good fortune. And so was the obsidian

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work. Both of those came together, and they were a great boost to the dissertation.

SMITH: Did you have hypotheses to test when you started out?

RENFREW: No, I must say, absolutely not. Nor do I think now that hypothesis testing is always the way research proceeds. I mean, there *were* questions to answer with all this stuff from the early bronze age of the Cyclades: What is this stuff?

Which is early, which is late? Are there separate cultures to be distinguished? What is the nature of the traditions? How did it start? What were the antecedents? Then you go and find some neolithic evidence, and that helps to answer the question. So that was finding material that was relevant to the question. I suppose, if one wanted to frame it in hypothesis testing—I've never thought of putting it this way—the hypothesis would be: with so flourishing an early bronze age, surely there were inhabitants in the Cyclades prior to that time. So, okay, you travel around and find something and you've confirmed the hypothesis. But, really, the question is, *what* was happening before that time?

In other words, I think it's fair to say that in many ways the richer question is one which requires an answer which can be one of hundreds of possibilities. When one says, "Frame a hypothesis," that rather suggests that you can get a yes/no answer, doesn't it? A hypothesis is either going to be confirmed or at any rate sustained, or refuted, so framing a hypothesis really means yes or no, though there may be more to be said. But the question was, was there a neolithic and what was it like? And we

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were able to say what it was like.

SMITH: Did you and Evans have different interests during this excavation, so you would divide up the labor?

RENFREW: To some extent. He and I both would have a similar outlook as prehistorians, a similar field interest. He had done brilliant work on prehistoric Malta, and then more recently had excavated at Knossos and had done some very fundamental studies of the neolithic there. In particular he had studied the pottery. I think we were both interested in everything and in all aspects, including environmental ones and so on, but when it was a question of who would study what, it seemed natural to him and entirely acceptable to me that he would do the detailed study on the pottery. I coordinated and did much of the description of the other finds, of which the obsidian industry was one important component; it required some special procedures. But I also worked on the other finds, sometimes described as small finds or special finds—tools and other materials, figurines all that sort of thing. Then we had specialists to do bone, and I was able to persuade somebody whom I knew already in Cambridge, Nick [N. J.] Shackleton, to look at the molluscan remains, which were numerous. My wife Jane took on the fish remains, which turned out to be very important, because there were great quantities of tuna, and she was already a specialist in animal bones at that time. She went to the British Museum of Natural History and spent days locked up, studying the collections of skeletal fish to develop



the necessary specialism.

SMITH: Was there any paleobotany done?

RENFREW: Yes. We had somebody out looking for pollen, but she didn't find it. I think I'm right in recollecting that Jane was already embarking by then on her own specialism in paleobotany, so she looked at all the macro remains, which were preserved in a rather unusual way; they were actually fossilized, which is unusual on archaeological sites which are relatively recent, four or five thousand years old. So they weren't carbonized remains in the main, though we may have found some, they were actually in the clay; they were silica skeletons of the grains, so she was able to study those very effectively.

SMITH: To what degree did your studies involve specialization? As you were moving towards formulating the project at Saliagos and thinking through what the issues were that were important to you, were you getting channeled into specializing in specific issues?

RENFREW: Well, I think it's fair to say that issues kept on cropping up, and it's probably true that the pattern of my work has been to find these issues, which then, as one examines them, lead on to other issues. In the case of the Cyclades, one very good example would be the issue of obsidian, because when I decided to work in the Cyclades, much of the interest was going to be in the contacts between the Cyclades and other areas, about which so much had been written, and about much of which I



was skeptical.

SMITH: You were already skeptical?

RENFREW: Yes, that's right. Indeed, I think it's worth going back to talk about skepticism. I said I was encouraged in my skepticism by that excellent physics master who was always willing, if one was talking about physics, to show how it should be demonstrated by experiment or by hard theory. He was always willing to be tested and challenged. It was often quite challenging to try and show that he was wrong on something. As I said before, he very rarely was, but he was very willing to admit the likelihood that he might be. One of my first doses of skepticism was in the summer of 1962, when I first went round east Europe with John Nandris, whom I met on the Nea Nikomedia excavation, and a Danish friend, Waby Armfelt, who was able to get hold of a Volvo car. We drove through East Germany from Denmark, and then Poland, and Hungary, and we also visited Romania and Bulgaria and Czechoslovakia.

I became very intrigued by the very rich prehistoric remains of the Copper Age in Bulgaria and Romania. We visited the wonderful tell site of Karanovo, which was a real lesson, a great eleven-meter section. Looking at that, it became clear to me there were strange misunderstandings about the chronology. Already while doing my undergraduate degree with Glyn Daniel, I had been writing about megaliths, for instance, suggesting that perhaps the megaliths of Iberia might be independent from the supposed Aegean connections. Indeed, I also had become skeptical about the



relationship between prehistoric Britain in the early bronze age, the so-called Wessex culture, and the Aegean, so I had a lot of broader questions which arose from the Cambridge degree course, really. I was uneasy about this notion that the megalith builders of western Europe came from the Aegean, which was one of Glyn Daniel's great interests.

SMITH: Meaning that this was something that he believed in?

RENFREW: He did believe in it. He had written an influential paper in 1938, when he had documented this. It was in the 1960's that the first radio carbon dates were coming through, particularly for southeast Europe. Some of the dates were coming out earlier than they were predicted to do, and the view that these cultures of southeast Europe were derived from the Aegean somehow didn't seem to make very good sense. Already, one or two good articles, for instance, one by James Mellaart on the Copper Age of southeast Europe, were saying that these things were a lot earlier than we had thought. The aim had always been to situate the Cyclades in European prehistory and review their impact on European prehistory. During holidays, in the time I was doing my undergraduate work, I had been with my father to Spain, for instance, and looked at megalithic tombs there, and I had come to feel they really didn't have much to do with the Aegean, so I had seen these Spanish things, and I had seen these things in southeast Europe.

SMITH: Now, when you say that, was it a formal or a stylistic analysis?

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IN SENATE
JANUARY 1, 1901.
REPORT
OF THE
COMMISSIONERS OF THE LAND OFFICE
IN RESPONSE TO A RESOLUTION
PASSED BY THE SENATE
MAY 1, 1899.
ALBANY:
J. B. LEECH, STATE PRINTER.
1901.

RENFREW: When I had been to Spain I had seen the Spanish passage graves and come to admire them very much. I thought they were stupendous structures, and very exciting, and the plain fact is, they didn't resemble the Cycladic tombs that they were sometimes said to resemble. I had also been to prehistoric Malta and didn't see anything very much like the Aegean there. Then, looking at all the figurines and these rich materials in southeast Europe, although many detailed parallels had been drawn between those objects and Cycladic objects, most of them clearly were not close resemblances. Because I had seen original materials in Spain and in southeast Europe, and because I knew the Cycladic material by then very well, I could see that they weren't particularly similar. So, already, that was an important part of the dissertation.

Now, we were beginning to talk about obsidian, and you were asking me about specializing. Part of the project was clearly to see to what extent there had been trade between the Cyclades and southeast Europe and Spain. Had there been trade, you would have expected Cycladic obsidian would find its way to those places. Given that obsidian was the major traded commodity in the Aegean before the bronze age, and given that much of it seemed to come from Melos, in the Cycladic islands, it was clear that that was going to be a project in itself which required a degree of specialism, focusing on the obsidian. I asked myself how one was going to use scientific techniques to document that. I had already studied the work that had been

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done in British prehistory on faience beads by means of trace element analysis, which hadn't been very conclusive, although that was the first application of trace element analysis by optical emission spectroscopy to such a problem. Some trace element work had also been done in metallurgy, but not very successfully. So it was as a result of that that I consulted Joe Cann, whom I mentioned earlier, who was by that time a research fellow at St. John's College. I asked what could one do about obsidian to determine its place of origin. He saw very clearly that this was a straightforward problem. We had some knowledge of the potential sources of obsidian, mainly Melos, but there were others in the Aegean and beyond, and in Italy certainly, at Lipari. So Joe and I started looking at some techniques, and the first one to look at was refractive index. We took measurements of the refractive indices of different obsidians and found there really wasn't very much variation; it just wasn't a very suitable technique. And specific gravity wasn't a suitable technique, and thin section analysis wasn't a suitable technique, so trace element analysis seemed to be the way to proceed.

It was very good fun, because we had to obtain samples from all over the place. The Museum of Mineralogy here had a very good collection of minerals, and I wrote away to get archaeological samples from different places. I collected quite a few from Greece myself and secured permission, and then we sat down and we ground them up in a pestle and mortar and put the powder in a tube, and then Joe,



with a technician in that department, put them through the optical emission spectrometer, which produced photographic plates which Joe read, so that he was doing a lot of the actual measurement work. Then we sat down together to look at the data, spent a lot of time plotting out data, and it turned out to be a problem that did have a resolution. You could distinguish many of the sources by looking at the trace elements: barium and zirconium and so on. Joe and I probably had done experiments together at school in the sixth form. It wasn't very different, really, from what Mr. Marshall might have said, although this was chemistry now and not physics, and here really interesting results were coming through. It was just very straightforward application of science, though of course Joe was using more sophisticated instrumentation which I wasn't all that familiar with. It led to very clear-cut results for the Mediterranean. Then there's always the question, are there sources you don't know of? That's always the issue, so that had to be considered.

[Tape II, Side One]

RENFREW: We considered the obsidian sources of Europe and those of Anatolia, and indeed whether there were any in the Near East. It turns out there aren't, but that was a big issue. So we had to do all of that to get an answer, even though the initial question may have related to Melian obsidian. In no time at all we had done a piece of work which was relating to the obsidian trade in the Old World, and we prepared that into a paper and a lecture. Grahame Clark was at that time the editor of the



Proceedings of the Prehistoric Society, and he was enormously positive about our work. He could see the importance of it, because we really were documenting trade routes on a large scale, and this had been about the first time that a characterization method had been devoted successfully to this. It had been tried with metals, but not really very effectively.

Grahame Clark arranged that we should be invited to lecture on this to the Prehistoric Society. I was still a research student at this time, it was before I'd written my doctoral dissertation; Joe already had his doctorate in geology. So we gave our first formal lecture, which was very exciting, and it led to continuing work. The next paper we did was on the early neolithic of the Near East, which had nothing to do with the Aegean, but it was clear that there were all these sites in the Near East where obsidian was used, and we could get material by writing to the excavators. The obsidian sources, it seemed, were a long way away; they were all up in Anatolia. We had to confirm that this was so, and indeed it did prove to be so, and that gave a whole picture about trade in the early neolithic that simply hadn't been understood or known about before. Then there was spin-off for the west Mediterranean, so it was a piece of work that developed its own momentum.

We could have gone further. We were tempted to look into obsidian in the Americas, but Joe said, "Oh, we mustn't do too much at once." Had we done so, we'd have been writing the pioneering papers which hadn't at that time been written



on the obsidian trade in the Americas, but I think Joe was probably right: better to make a more complete picture, which we did. I had always been interested in trade as an indicator of contact, because, as I was saying, the broader problem was, what was the role of the Cyclades in European prehistory, so what contacts did the Cyclades enjoy? One could be skeptical about Spain, and the way to answer that question was by asking what contact could actually be documented, not what you thought was there by resemblances. If you found a bit of Melian obsidian in Spain in a specific context, then you knew that people from Melos or from somewhere around there were going to Spain with their obsidian. No such piece of obsidian has ever been found in Iberia. So that's one strong reason for thinking that all this talk about contacts between the Cyclades and Iberia is probably erroneous. In terms of evidence one could say, "Look, if there were these strong contacts, would there not be Melian obsidian in Spain?" which there probably would be. But no piece has up to now ever been found.

So that piece of characterization study reinforced one's feeling that this was the right way to go, and it led to more work on trade. One of the first theoretical papers I ever wrote was a paper in *Current Anthropology* called "Trade and Culture Process in European Prehistory." Again, it started from that experience with obsidian, but looked at other materials also. This was by now some years after I'd finished my dissertation on the Cyclades, and in exercising skepticism about these

[Faint, illegible text visible through the paper, likely bleed-through from the reverse side. The text appears to be organized into several paragraphs and possibly a list or table structure.]

links and asking for strong evidence in the form of direct proof of contact it was generalizing the idea, somewhat. So it was a specialism which then resulted in broadening out, or perhaps generalizing a little from the initial ideas.

SMITH: Of course, since you were skeptical, the results may not have come as a surprise to you.

RENFREW: That's right.

SMITH: And at this time you become known as a person who was tackling this question of diffusion from a negative perspective.

RENFREW: That's right, and to link that to what I was just saying, I think because the Cycladic case showed skepticism to be warranted, in particular in relation to Iberia, and then in relation to southeast Europe, it led on to the Sitagroi excavation, which, again, was perhaps a reinforcing experience. Just as you go on to do more work on obsidian if you're getting really good results from it, so, likewise, if the idea was that these contacts were illusory, that they didn't exist, and you couldn't find any hard proof for them in relation to the Cyclades and Spain and southeast Europe, certainly that made one well predisposed to question more firmly the links between Wessex and Mycenae. There it required some radiocarbon dates to show that things could be seen differently, whereas in Iberia and in southeast Europe, although the radiocarbon dates later completely sustained the skepticism, the skepticism was there without the dating. You could just see the links weren't very good.



SMITH: To what degree was this critical rethinking about the issue of diffusion a personal or idiosyncratic interest, and to what degree was it reflecting a broader questioning of the data that existed, either amongst the students or both students and faculty?

RENFREW: I think probably it would be fair to say it was more personal. When it was agreed I would read archaeology for part two, I had completed my work in the sciences, so I came up early and had a long summer's reading here in Cambridge to try and do some of the basic work that I wouldn't have done in part one. All the other students would already have done a year of archaeology and anthropology. I read quite a few books with care, among them some of Gordon Childe's books, which were undoubtedly the most useful. He was a very clear-thinking man and went down to issues of principle, but I got very skeptical about his diffusionist approach simply from reading his work carefully. Also it was at that time that I read his book *The Aryans*, where he developed his idea about the Indo-Europeans coming from the steppes, and at that time I found it totally unpersuasive; it just didn't seem to be good argument at all, and years later I returned to the issue.

So I think I could already see that a lot of the diffusionist arguments weren't very good ones, and that meant of course, without one's thinking it through in any profound way, that much of what was being said about European prehistory wasn't well founded, and the storyline in some cases didn't hold up very well. Whatever the



real story was, one wasn't getting it. So I think I did have that sense about things. I don't think there were any other students in Cambridge being particularly skeptical about those specific issues. Certainly David Clarke, who was by then a research student in Cambridge, was a person of great freshness of thought. I had supervisions from him, along with other contemporaries. I won't say he was particularly antidiffusionist or anything, but I think he was just very willing to think in fresh ways. There was a good climate in Cambridge. Grahame Clark always stressed the ecological approach, and indeed Eric Higgs developed that to an extreme degree, and that also involved being willing to think skeptically about what was being said. So although I don't think there was much skepticism about diffusion, particularly, there was plenty in the atmosphere that encouraged one to think things over in a fresh way. There was a very healthy climate in that respect.

SMITH: Did you at this time have some kind of working hunch that constructed an alternative story?

RENFREW: I haven't really thought very carefully about what my preconceptions would have been, but I think probably I would have held the feeling that this isn't really the way things change. Things don't change just by people rushing in from a great distance and setting up shop in this colonialist manner that had been suggested in Iberia, so therefore there must be other processes of change. I think when one is criticizing an existing hypothesis one puts a lot of one's energy into showing why it's

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wrong, so that one puts rather less effort into saying what really happened. But in my doctoral dissertation I did suggest some general ideas about why things changed in Iberia. They changed through local processes, and I'm sure I spoke of demographic processes and economic and social processes, without having really a great deal of flesh to fill in on those bones.



SESSION TWO: 16 MAY, 1996

[Tape III, Side One]

SMITH: When we left off yesterday, I had a question that I wanted to ask you about parsimonious explanation and its role in archaeology, which is both a historical science in some ways but also a nonhistorical science, or a true social science.

Diffusion had the benefit it seems to me of a more parsimonious explanation of these events than multiple origins. You then had datings from radiocarbon and so forth that made that explanation questionable. One alternative would be to say, "Well, there must be material that's missing. We have to go back and look at the record again. There must be earlier Greek materials or Cycladic materials," or whatever. You began to talk about social process or processual explanation, which provides an alternative parsimony, but it seems to me that at this time you didn't quite yet have all the bits and pieces of the processual paradigm in place.

RENFREW: Well, in that last remark I'm sure you're right, nor do we now, but just to go back to your first point about explanation: parsimony is a very important principle, but there is another overriding principle for explanation; namely, that it shouldn't be demonstrably wrong. If we're talking about science, the refutationist principle is quite a high one. I mean, it's true of most fundamentalist explanations that they have the benefit of parsimony because they very often appeal to what is in effect a single principle, that it's all God's work, it's all for the best, or whatever. If we're

1. The first part of the report discusses the importance of maintaining accurate records of all transactions. It emphasizes that proper record-keeping is essential for the integrity of the financial system and for the ability to detect and prevent fraud.

2. The second part of the report describes the various methods used to collect and analyze data. It includes a detailed discussion of the sampling techniques employed and the statistical methods used to interpret the results.

3. The third part of the report presents the findings of the study. It shows that there is a significant correlation between the variables studied, and that the results are consistent with the hypotheses proposed.

4. The fourth part of the report discusses the implications of the findings for policy and practice. It suggests that the results of the study can be used to inform decision-making and to develop more effective strategies for managing the system.

5. The fifth part of the report concludes the study and provides a summary of the key points. It also includes a list of references and a list of figures and tables.

talking as we were yesterday about mathematical proof, a good proof is a simple parsimonious proof, but that is perhaps secondary to the circumstance that the proof, if we're talking about mathematics, should be right, that is to say, not demonstrably in error. I think the same would apply to diffusion. Just to appeal to an agreeable principle doesn't really get us very far if it is at variance with the facts. You're quite right, however, that at any given time the facts are not always clear. I'm not saying that archaeology is like a hard science, but it is true when one looks at many situations in science where you have an existing explanation, and then it doesn't seem to be working very well, you have a period of doubt or confusion, and then someone is able to produce a new paradigm that somehow reorders the materials and seems to have its own coherence, and, if you like, its own parsimony, but it also accounts for the facts better. [Thomas] Kuhn focuses on this issue when speaking of a paradigm shift.

We don't have to adopt a Kuhnian view of paradigm shift necessarily, but very often it is quite appropriate if we're talking about questioning a guiding principle. For instance, I find that in some discussions about the origins of language families, sometimes one is offered the center-of-gravity principle which linguists allegedly like to follow, that if you have a language family and you are looking for an origin, you should look for the point of origin near the middle rather than on the edge, again in response to some notional principle of least movement. Again, it's a kind of



parsimony view. A lot of linguists actually appeal to this in order to override suggestions of explanations to the contrary. If you actually ask them what would underlie this principle that should be in the middle of the area, they begin to get very vague, and indeed appeal to notions of parsimony. You don't get any very clear answer.

One way to approach the notion of parsimony in relation to diffusion is to ask, "Could somebody explain to us how the diffusion principle is supposed to work?" And that gets you back to the idea of innovation that has indeed been said of metallurgy, that it is so complicated a process that it could only have been invented once in human history. Then you have to ask yourself in what circumstances would it be invented once, and is it so implausible that comparable circumstances could occur elsewhere? This was a dispute a century ago when anthropologists were talking about very general things—painting faces or tattooing or mummification. You've got mummies in Egypt, and you've got what are said to be mummies in Peru; does that mean the one came from the other? Which was first?

I think few would deny that pottery-making was invented in different places at different times, so ultimately you do require some debate about the origins of technologies, about innovation and so on, and when you do so, then the so-called principle of diffusion is seen to be an erroneous principle. If it's being used to suggest that it's very difficult for something to be invented more than once, one would have to



ask, "What is the principle of diffusion? What is being asserted?" When it is alleged that a diffusionist explanation is more parsimonious, we should then ask, "Could you kindly give a formulation of this general principle of diffusion?" I don't think there is such a formulation; nobody's attempted a successful one. However, that doesn't get away from the other point you were making, that if you have a generally accepted explanation, and then you have some data that begin to look as if they're undermining it, but not very conclusively yet, when the chronologies are not well established, where do you go from there? It was the anthropologist, Julian Steward, who pointed out that theories don't die, they are replaced by other theories. This is, in a way, a pre-Kuhnian formulation of the Kuhnian view, I think.

So, yes, it is incumbent on one to offer a better explanation. But, for instance, if we're talking about the construction of megalithic monuments, to suggest that this phenomenon might have happened in Iberia is not an outrageous suggestion in itself, though it may require further elaboration. One does have to ask, what are the factors that lead to such phenomena? Interestingly, that particular question is still one which is quite problematic, because the megalithic monuments of western Europe were constructed in the northwest, in what looks like a unitary area; we do seem to have multiple origins there, and still nobody has really come up with a very clear account of what was going on. Was there just one origin in western Europe, or several? I don't think anybody really thinks anymore that this phenomenon originated in southeast



Europe.

If the diffusionist principle is that every invention was made once only, first of all it's a silly idea. How would it get made in the first place if it's so difficult? It was Lord Raglan, I think, who said that everything begins at Sumer, but what was the Sumerian secret that everything should begin there? So it's not a good principle, really, and this is sometimes overlooked. But you're right that alternative processual explanations had not been worked out, and indeed are still not well worked out in most areas, and that is really the main blame we can lay at the door of diffusionist thought. For half a century it has prevented intelligent, coherent thoughtful discussion of the nature of change; moreover, it survives today.

One of my irritations as an archaeologist is that there is a group of people who invoke the world systems view of [Immanuel] Wallerstein, which of course was formulated for the seventeenth and eighteenth centuries A.D. I'm not criticizing it as a principle of economic analysis of trade over that time period, but they somehow lift it and put it in the prehistoric past, and they effectively paraphrase the Childean model, which one had thought over the past twenty years has been refuted as a general model. They paraphrase it in world systems terms and talk about center and periphery, and the entire discussion goes back exactly to where it was, in my view, thirty or forty years ago. It's interesting that the diffusionist principle will not lay down and die. So there's a short diatribe on the matter for you! I'm not sure if I



answered your specific questions about the situation in the early sixties of the balance of the evidence against the theoretical issues, but your main point is right, that processual explanations were not well developed and indeed are still not well developed.

SMITH: Perhaps I'm wrong here, but in terms of your own personal knowledge, processual explanations were not yet part of your theoretical armature in the early sixties, were they? Weren't you still working your way through the Childean archaeological model?

RENFREW: I wouldn't quite say that. If one's asked to define what one means by "processual," it's not very difficult to say one's talking about processes of change: social, economic, and demographic. So what does that mean? All that means is that we're talking about change without very strong preconceptions. So, really, one's just trying to sit down and discuss what makes societies change, without bringing some already constructed principles from outside—diffusionist or whatever they may be. I think any archaeologist ought to be interested in change.

One element about the processual which perhaps differs from some currents of thought is that one is hoping to see some more general principles, to try and have some insights into change in a general sense, to the extent that some of those who are more particularist would say, "I'm not interested in talking about change, I'm interested in what happened in this place." What were the influences, who were the

1. The first part of the paper discusses the importance of maintaining accurate records in the field of environmental science. It highlights the challenges faced by researchers in collecting and organizing data, and emphasizes the need for standardized protocols to ensure the reliability and comparability of results.

2. The second part of the paper presents a detailed analysis of the data collected over a period of six months. It includes a series of tables and graphs that illustrate the trends and patterns observed in the study. The analysis shows that there is a significant correlation between the variables studied, and that the data supports the hypothesis that was proposed at the beginning of the paper.

3. The third part of the paper discusses the implications of the findings for future research and for the management of natural resources. It suggests that the results of the study can be used to inform policy decisions and to guide the development of conservation programs. It also identifies areas where further research is needed to better understand the complex interactions between the different components of the ecosystem.

4. The final part of the paper provides a conclusion and a summary of the key findings. It reiterates the importance of accurate record-keeping and the need for continued research in this field. It also expresses the hope that the findings of the study will be useful to other researchers and to the public alike.

people, what were the events that led to what happened next?" There is a very particularist view of history that would somehow rather shy away from any statement of general trends or general principles. Well, I don't take that view; I think it begins to be interesting when you hit on some factors which seem to apply in more than one place and give you some insights into change on a wider level. And I'm sure that was part of my aspiration at the time.

We were talking about scientific background. If you spend some time looking at how factors work together, you do have some notions of explanation whereby you do hope to see causal factors operating, maybe even which you can quantify. So I think that was a natural outlook, but the strange thing is, there still isn't very extensive literature on these matters. Obviously, in the social sciences people have been more inclined to look at economic factors and their working on history, but that takes us into a slightly different approach. The *Annales* school had trod very similar ground already, really.

SMITH: At this point, how much reading were you doing in social scientific theory?

RENFREW: Very little. I'm ashamed to say that I've never really read very much in that area. Mainly because people have relied so heavily on Marx, I've sat down and read what Marx said, which on many issues seemed to be very coherent. There are things one disagrees with, about the universal nature of the class struggle and so on, but many analyses just seem to be insights which nobody could doubt today. Nobody

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 This is essential for the proper management of the
 business and for the preparation of the annual
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does doubt his generalizations about the sociology of knowledge, not even the strongest anti-Marxists. Grahame Clark was always politically and personally a rather conservative figure, but in writing about economic prehistory, many of the things he said certainly wouldn't run counter to many of the things that Marx wrote.

SMITH: I wanted to ask you, what were the particular reactions of Clark and Glyn Daniel to your work at this point on the critique of diffusionism?

RENFREW: Grahame Clark was always unimpressed by the minutiae of culture history; he was always interested in the underlying economic processes. So there is no doubt that you could regard him, as indeed you could also regard Childe, when he was writing about the urban revolution, as an early processualist thinker. [Clark] grew impatient with long discussions about the typology of megaliths. He and Glyn Daniel were intellectually always somewhat at loggerheads. Clark would be dismissive of discussions of megalithic origins, partly I think because that was one of Glyn Daniel's main subjects.

I remember, in my book, *Before Civilisation* [: *The Radiocarbon Revolution and Prehistoric Europe*], I took as a good expression of the general view of culture change in Europe in the later neolithic a passage from Grahame Clark's book *World Prehistory*, and I think this particular exemplification met with his disapproval, because I remember him making a very dismissive remark about my book. I think he was just mildly miffed that I had criticized a passage in his book. It was not one of

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the more important sections. He was just summarizing what was generally known and accepted, and since I was disagreeing with what was generally known and accepted, I was presuming to correct what he had written, so he was mildly miffed by that.

In Glyn's case it was different, because he had invested much effort into discussing mainly megalithic origins in what was a diffusionist framework, so I was therefore contradicting his own work much more systematically. On the other hand, as editor of *Antiquity*, he had been following over the years new radiocarbon dates, calling into question very early radiocarbon dates in Brittany, for instance, and he began to see the writing on the wall there. So I think he was actually quite intrigued to see other views coming forward that would somehow make sense of the radiocarbon dates and bring them into a coherent picture, which was indeed what was happening. So I think he was remarkably tolerant; he wasn't angry about this, and he was willing to accept most of my writings for *Antiquity*. He accepted my paper on the Iberian megaliths, which was called "Colonialism and Megalithismus," and I think he was really rather generous and open-minded about it. Indeed, my other paper, "The Autonomy of the South-East European Copper Age," was accepted by Grahame Clark, who was editor of *The Proceedings of the Prehistoric Society*. So I think in both cases their reactions were generous.

There were some slightly less generous reactions. I wrote a paper called



"Wessex Without Mycenae," which was one of the first of these papers calling into question the diffusionist paradigm. It was relevant in England because it was coming close to home, asserting that Stonehenge was built without Mycenaean influence. I submitted that to *Antiquity*, and Glyn followed his usual course of sending it out to one or more people to read. This was really before the days when external refereeing was absolutely the norm; it just happened that the editor wanted to do it. So he sent it to Professor Richard Atkinson, who was an authority on Stonehenge, and he came back with a very critical review, which I think Glyn did show me at some point, later. Certainly from my standpoint, one could form the view that Atkinson had objected to my paper because he didn't like being criticized in this way.

So that article was rejected by *Antiquity*, but fortunately I was in a position to submit it to the Annual of the British School at Athens. I was in the department of Ancient History at Sheffield at that time, and the head of department, Professor Robert Hopper, was the editor of the Annual, and that perhaps facilitated the acceptance of my article. It had quite a big impact. In general, I have to say that the response in the British archaeological world was a generous one. People may have disagreed or been shocked, but they didn't say, "What terrible rubbish. This should not be published." In general that was not their view. I think there's quite often a tendency when an established view is being challenged that the people doing the challenging are somehow denied access for their views, to some extent. It shouldn't

1. The first part of the paper discusses the importance of maintaining accurate records of all transactions and the role of the accounting system in providing reliable financial information.

2. The second part of the paper examines the various methods used to collect and analyze data, including surveys, interviews, and focus groups.

3. The third part of the paper describes the results of the study, which show that the accounting system is a critical component of the organization's financial management.

4. The fourth part of the paper discusses the implications of the findings for the organization and provides recommendations for improving the accounting system.

5. The fifth part of the paper concludes the study and summarizes the key findings.

6. The sixth part of the paper discusses the limitations of the study and suggests areas for future research.

7. The seventh part of the paper provides a list of references and a list of figures and tables.

8. The eighth part of the paper provides a list of appendices and a list of footnotes.

9. The ninth part of the paper provides a list of glossary and a list of abbreviations.

10. The tenth part of the paper provides a list of acknowledgments and a list of contact information.

happen, but it sometimes does. That really didn't happen in this case; people were positive and interested. Partly of course, I think it is incumbent on one to try and exercise tact when one disagrees with established figures, but that doesn't prevent one from disagreeing with them. So I always thought I had a very fair hearing, and certainly these views didn't impair my continuing good relations with Grahame Clark or Glyn Daniel, both of whom remained very firm friends.

SMITH: In your work on the Cyclades, besides the question of diffusion you are also dealing with the question of urbanization. To what degree were those two linked questions?

RENFREW: I think they were linked questions. I think it's important, in all these discussions, to distinguish between the levels that one is working at. We were talking earlier about the Cyclades and about the interaction of the theoretical and the factual, and some of the theoretical issues arose when we were talking about the wider relations of the Cyclades, but I was emphasizing that in the Cyclades the work was to establish a culture sequence, and that wasn't really testing hypotheses, it was simply asking, What are the facts? It was data gathering, and ordering the data in a coherent shape. To some extent the same applies to the urbanization question. In other words, in the Cyclades, one was interested in saying, "We have these sites. What are their antecedents? What more can we find out, by fieldwork and by excavation, about the culture sequence? When did metallurgy arise?"

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I haven't really thought very carefully about the question of levels of analysis when one is actually doing detailed research. I think there would be something to develop there. I emphasized that the early bronze age was a time of great change with an internal dynamic in the Aegean, which seemed to arise partly from the development of metallurgy. Metallurgy may or may not have come in from outside, but as it built up it was largely an autonomous process using local resources in the main. And there were also agricultural changes which did link with the development of vine and olive cultivation and developing a broader spectrum of resources. So I was emphasizing, therefore, the importance of the third millennium, *before* the palaces of Crete came about, which was around 2000 B.C., and that I think made it easier for me to see that there were local trajectories of growth which permitted the emergence of a palace society. This made it possible to suggest that the emergence of palace society in Crete and then in Mycenae was essentially a process that was the product of a local dynamic. Until then I think the notion was rarely questioned that they were just implants from the Near East or from Egypt, where palace societies had already developed.

So I had to show that the foundations were already there in the third millennium. It's still of course possible to argue, and recently people have come back to the view, that the Minoan palaces would not have come about had there not been palaces already in the Near East. This is part of the resurgence of diffusionism, yet

1. The first part of the paper is devoted to a general discussion of the problem.

2. The second part is devoted to a detailed analysis of the case.

3. The third part is devoted to a discussion of the results.

4. The fourth part is devoted to a discussion of the conclusions.

5. The fifth part is devoted to a discussion of the future work.

6. The sixth part is devoted to a discussion of the references.

7. The seventh part is devoted to a discussion of the acknowledgments.

8. The eighth part is devoted to a discussion of the appendix.

9. The ninth part is devoted to a discussion of the bibliography.

10. The tenth part is devoted to a discussion of the index.

11. The eleventh part is devoted to a discussion of the summary.

12. The twelfth part is devoted to a discussion of the conclusion.

13. The thirteenth part is devoted to a discussion of the future work.

14. The fourteenth part is devoted to a discussion of the references.

15. The fifteenth part is devoted to a discussion of the acknowledgments.

16. The sixteenth part is devoted to a discussion of the appendix.

17. The seventeenth part is devoted to a discussion of the bibliography.

18. The eighteenth part is devoted to a discussion of the index.

19. The nineteenth part is devoted to a discussion of the summary.

20. The twentieth part is devoted to a discussion of the conclusion.

it's actually unfair to call it diffusionist thinking; I'm not trying to say it's careless application of an invalid principle. It's perfectly reasonable to argue that in a specific way, what was happening in the Near East had an influence on the Aegean, and though that would be diffusion, it's not the application of some vague and invalid principle, as it were. So that still remains a controversy, but I think it became possible to suggest that Aegean civilization was largely an autonomous product. That had to be argued on the basis of what happened before that palace civilization arose, and therefore to study what happened in the third millennium was central to that argumentation.

SMITH: Now, as you developed a thesis of indigenous developments, which seems to become a generalized principle, what kinds of changes did that force in methodology?

RENFREW: That's an interesting question. I think there were some elements of existing methodology which slotted in very well, and there are others that perhaps haven't still been worked through very effectively. One area where the existing methodology harmonized very well was the increasing interest at that time on economic archaeology, and particularly on the subsistence base. In the postwar era there was a huge development in the study of plant and animal remains. Part of it related to interest in the neolithic revolution, which grew very much in the forties and fifties, and the emphasis on economic and environmental archaeology was part of



that. So to understand what were the environmental factors, what was the habitat, what was the climate, what were the flora, what were the fauna—all that was going on very effectively already in the fifties and sixties before the so-called "New Archaeology" came about, and before these diffusionist issues were called into question. It was all there, ready for people to study the subsistence base more effectively.

What has been interesting is that in more recent years people have been looking at the social factors, in other words, the way these resources are used by human societies: questions of exchange and questions of storage. They're still economic questions, but if we're talking about storage, it's not just a question of production, it's how these things are being used, how they're exchanged between communities, and how this exchange articulates the relationship between communities. In my own explanation for the rise of civilization in the Aegean, I laid emphasis on redistribution, which is again allocation of resources in various ways. So if you're taking a processual view or an indigenous view, it's very natural that you're going to look very carefully at the resource base, first of all the food resource base.

Secondly, one looks with new interest at technological innovations, and some of the most important have been in the area of metallurgy, so the development of early metallurgy is something that fits naturally. If you're taking this sort of outlook, the possible *local* origins of metallurgy become an important field for investigation,



and this has been increasingly a major field in archaeology. Another factor in the growth of archaeological method is simply the investigatory techniques available. If you have a new technique, then it will be used, and you'll have to find something to do with the data. In a way, the study of trade, which we were discussing earlier, falls in that bracket. Optical emission spectroscopy could only be applied when the techniques were available, and they were soon superseded by neutron activation analysis, which opened new avenues for characterization studies, and other techniques for characterizing materials have emerged since. So this has made possible, and in that sense led to a huge development in studies of early trade, which is now a major field in archaeology.

But the point is, to return to your question, these are issues that you would wish to ask about if you were taking a processual approach. You very much want to know what was the contact between communities, and what was the flow of trade. What you also ought to wish to know, and this is where progress has not been so rapid, is what was the nature of society? If you want to ask social questions about social structure, there are no ready methods that at once throw up data in the same way. In the case of trade you just say, "Right! We'll analyze a bunch of potsherds, analyze a few metal artifacts, do some trace element analysis. Now, where are we? We'll do some statistical analyses, some principal components analyses, and we'll come up with some answers." But there are no standard procedures for elucidating



social structure, so the field of social archaeology hasn't had the same momentum, though there is the interest there, and indeed that was where much of the interest focused in the sixties and seventies. If we're trying to think about the development of societies in processual terms, what were the social changes? That links with demography, and *there* is an area where of course the development of techniques of site survey, field walking and so on, have been very significant—the use of surface archaeology rather than excavation. You get information about site distribution that wasn't otherwise available. So these techniques have been very much called into being by the desire to answer demographic questions.

But the area of study which you would think would have been developed quite early, that is, thinking about how societies articulate information, thinking more at a cognitive and symbolic level, has been slower to advance, partly because there are no self-evident methodologies. I think it's interesting that one of the ways archaeology has developed has been to use archaeological science. Whenever a technique is available that has an obvious use, people have used it, so lots and lots of subfields of archaeological science have come into existence because the methods are there. Yes, you can think of ways the data might be interesting, but very often it's not asking questions and then finding the answers, it's actually having the data first; it's like characters in search of an author, as it were, and that has been part of the history of archaeology in recent years, so it's been rather lopsided. Archaeological science has



developed, but other issues that matter as much or more have not been so coherently explored.

SMITH: You did say yesterday, and again today, that at Saliagos your primary task was to find out what was there, which sounds like a traditional archaeological excavation purpose, but you have written that the dig at Sitagroi was to test a hypothesis, which seems like a very different set of conditions. I wonder if you could discuss how you arrived at Sitagroi as a locale for testing your hypothesis, and were there different ways of digging and processing and analyzing that you applied at your second major dig?

RENFREW: Certainly. I'll just allude briefly to Saliagos again, if I may. If one is taking the sort of outlook we've just been discussing, then it does make one look at the evidence in a different way. For instance, Saliagos was one of the early sites in the Aegean where the animal bones and the plant remains were very systematically looked at and published. It was far from the first: I could mention German excavations in Thessaly as leading the way in that respect. But we did have a whole series of specialist techniques applied. We did try and source all of the materials, we did try and look at the environment, so there's a whole series of appendices, which perhaps did respond to the aspiration that one should be doing more than just digging up the pottery and the lithic artifacts and describing them. You are quite right that the interest of Saliagos was, as we were saying earlier, in seeing what happened



before the early bronze age. There was a blank on the map there; it was just data gathering, really, which as we've said, answered some interesting questions.

Turning to Sitagroi now, there was a problem that you could frame in hypothesis terms, and the issue was that one had the culture sequence for the Balkans—Yugoslavia, Romania, Bulgaria—well established as a sequence by a whole series of scholars, and you had the culture sequence for the southern Aegean very well established. It was the relationship between the two that was completely unclear, and was indeed the focus of controversy. It was generally believed that Troy phase II, as it were, was the source of much that was happening in the Balkan Copper Age. This meant that those cultures of the Balkan Copper Age—the Vinča culture, Gumelnița, Karanovo VI—would have to be contemporary with, but ultimately slightly later than Troy II for the generally accepted explanation of Aegean origins—for metallurgy, for the figurines, for the signs of a protoscript in the Vinča culture—for all these things to be valid.

So the basic explanations and ideas could be reduced to quite a simple factual question, and indeed a hypothesis. The existing hypothesis was that these cultures essentially derived from Troy II and its contemporaries and were therefore contemporary with and later than Troy II, whereas it was clear to me, and indeed as I mentioned earlier it had been suggested by James Mellaart, using a few radiocarbon dates, that somehow they seemed to be earlier, and so there was something that

[Faint, illegible text, likely bleed-through from the reverse side of the page]

wasn't working out there. Because of my work in the Cyclades, I had become completely skeptical about most of the supposed links, many of which were expressed in Cycladic terms: the figurines of the Balkans were supposed to be local barbarian reflections of the Cycladic figurines. Also, there was a lot of talk about spirals; you have spiral decorations on some of the pots in the Cyclades, and you have spiral decorations on pottery in the Balkans. There was a very extensive literature, some of it going back to the last century, about the origins of the spiral. Well, if you have a slightly skeptical turn of mind, you could well ask, mightn't somebody invent a spiral design independently? But that was not the question that was often asked. Talking about the application of rather crude diffusionist principles, there were maps showing the diffusion of the spiral as a motif from one part of Europe to another.

So there was an interesting question, and the solution to it already began to surface when one looked at some of the publications from the thirties and forties. Pottery, recognizably of a Romanian and Bulgarian type, broadly Gumelnița culture pottery with graphite decoration, was turning up in sites in Macedonia. It was usually in surface collections, and just in a few cases in excavations, but they were excavations that didn't have very good stratigraphy—nothing that would be very coherent. But it was clear that in this region at the north of the Aegean, you seemed to have an overlap in the cultures: you had cultures recognizably of Aegean type, Troy itself and the contacts with the Cyclades, but you also had these Balkan cultures.



Then there had been some very good site surveys undertaken by David [H.] French, who became the director of the British Institute at Ankara. As a doctoral student he had done very systematic site surveys over a very wide area—north Greece, Turkey, Macedonia—and his dissertation and articles were full of good drawings of this sort of pottery, which really documented the possibility.

I went and visited all the sites in the north Aegean which had been mentioned in French's work and in other such works, and there were a number of them. I visited Sitagroi and found on the surface good quantities of pottery. You could at once see that some of it related to Karanovo phase III, and other bits related to Karanovo phase VI, so this was pottery essentially of Bulgarian type, but nonetheless in the Aegean basin, only twenty or thirty miles from the north Aegean coast. So it did seem very tempting indeed to try and look at the stratigraphy. Already, before digging there, I'd used the concept of the "fault line," just the suggestion that something seemed to have gone wrong with the relationships, so that maybe you had to move the Balkan cultures back together as a great block much earlier; that was what the radiocarbon dating was already suggesting, but people were criticizing the radiocarbon system and it wasn't entirely clear. So we did choose to dig at Sitagroi, and we got a wonderful stratigraphy of six or seven major phases which we were able to recognize. We obtained a series of radiocarbon dates, which gave a very clear chronological sequence, and that gave a fixed point in what had previously been a

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terribly fluid and muddled area. Our categorical demonstration of the chronological relationships involved was quite quickly accepted by the Balkan archaeologists, who previously had followed the "Troy comes first" idea.

In relation to your question of how that affected excavation techniques, in a way it led us to a very traditional approach. The question was chronological, so we were still arguing the old arguments and looking at old classes of data; this was the way all these discussions had been framed for the past fifty years. So we first of all obtained a very good stratigraphy. We sunk a shaft down through the center of the mound, a nine- or ten-meter shaft, and generated a wonderful sequence of radiocarbon dates from it. It was perhaps new that we were placing so much emphasis on good radiocarbon dates and good recovery of short-term plant remain (seeds), long-term plant remain (wood), and animal bones. That methodology was conventional, but it was done in a very systematic way.

We were also interested in other issues. We had a metallurgist working with us on the site and we obtained samples of crucibles and slags, and we were able to analyze those and see how far the metallurgy had developed. We also undertook a site survey to look for similar sites in the area, and that was a systematic piece of work. We were studying the trade through *Spondylus* shells and we had a related project on the origins of *Spondylus*. So we did move off in a number of directions. We had very good analyses of bone remains by Sándor Bökönyi, and plant remains by

1. The first part of the paper discusses the importance of the study of the history of the United States. It is argued that a knowledge of the past is essential for a full understanding of the present and for the development of a sound policy for the future. The author points out that the study of history is not only a means of satisfying a natural curiosity about the past, but also a means of developing a sense of responsibility for the future. He concludes that the study of history is a necessary part of a liberal education and that it should be made a compulsory part of the curriculum of all schools and colleges.

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Jane Renfrew.

[Tape III, Side Two]

SMITH: Were your methods providing information that could answer those questions about why changes were occurring, or would you have to adopt another approach?

RENFREW: That's quite difficult to say. First of all, I would say that we did develop methods to answer the chronological questions. We had this long sequence of layers, with pottery coming from them, and since that pottery had never been studied very much in an Aegean context, we had to develop our own sequence. We did use quantitative techniques which were very much drawn on those which John Evans had developed at Knossos. Evans's quantitative techniques of counting and weighing sherds and using sieving were very much applied at Sitagroi. But those were methods relating to the older questions.

How these cultures developed and changed are questions of a breadth which cannot be satisfactorily answered from a single site. Clearly, you do need environmental information, and you can look at the food supply question, partly because the techniques were so well developed. We had an excellent pollen specialist. It so happened there were wonderful pollen deposits near Sitagroi, some of which had already been used by climate historians, so we were able to do thorough work there. As I was saying, the food residues were very carefully looked at—the plant remains,



the animal remains, the smaller fauna, and so on. So we were able to think about how the exploitation of food resources developed. That you can do to some extent from a single site in its local environment. Through the site survey, we considered settlement patterns in the region in general, so that was beginning to move on to social questions, but from a single site it's very difficult to make good statements about regional demography.

When it comes to social organization, you would need a different excavation strategy. We in Sitagroi made it our primary objective to get the sequence straight, which I'm sure we did do. But with a large tell mound, if you want to look at the organization of the village, you really would need to excavate either the whole site or sample it in an effective way to look at changes in the settlement on the site itself. We did start a large area excavation, but it was slow work, and we only got down to a few levels. We indeed found good indications of houses, but they were quite difficult to recover. But then when we got a little lower down, we found evidence for a burnt house, and that gives much better preservation because the walls begin to be baked, so they are recovered in situ. The pottery is destroyed in the burning of the house, so that you have whole pots, or broken but restorable pots in situ. So we did have that experience, and I have sometimes thought that it would be nice to go back to that site. There are probably other houses that caught fire at the same time, so it wouldn't be difficult to do a large area excavation, and that is what is really needed.



On the other hand, if you are choosing a site to ask questions about social organization, you might well choose a single-period site with very good preservation so that you could get the complete settlement plan. You wouldn't necessarily choose to excavate on a deeply stratified site like Sitagroi, which has all the levels. Very often the upper levels, which you have to dig first, are much eroded, because they are on the top of a mound, so you might well *not* choose Sitagroi if you wanted to ask questions about social structure at a particular period. But you touch there on one of the real problems of archaeology. You can't run archaeology as you would run many scientific experiments, in terms of hypothesis testing, because in most scientific experiments, or many of them, you are after certain classes of data, and you simply ignore other classes of data that you might collect but don't, whereas when you are doing an archaeological excavation you are destroying levels, so if you are interested in a level a thousand years down in the mound, you can't really take a bulldozer and just bulldoze the rest off; you really have a responsibility to recover and record and publish what you find. That means that your responsibilities as an archaeologist prevent your following a purely Baconian approach, or Popperian if you want, of question and answer. They oblige you to take what you might call an inductivist approach, in the sense that you are obliged to recover these data and record them, even though you are not in the least interested in them and can't think what you'd do with them anyway.

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So that is really one of the paradoxes of archaeology. If you are doing it responsibly, you can't just follow a pattern of hypothesis testing. But if you want to move towards that, then you clearly choose a site which is not going to encumber you with too many data that you don't want. Certainly, if you are trying to follow various questions at once, it can lead to all kinds of problems. It's true our main question was culture sequence at Sitagroi, so you get a site with a wonderful culture sequence which we did do, but that at once lumbars you with difficulties if you are interested in *synchronic* analyses, and it's perhaps ironic that most studies of change require also a synchronic approach, where you really look at a number of dimensions of what is happening at a given time.

SMITH: Now, your next excavation was in the Orkneys, and there you did move on directly to discuss social structure. How did you choose that site, and again, had you in this case framed a question or hypothesis relating to the social structure of that society?

RENFREW: I have to tell you the anecdote of how it came about. Again, the initial interest was a chronological one, although one could at once see that it had to be followed up by social questions. I hadn't been to Orkney until I went there with a television crew. I had made some radio broadcasts and begun to publish articles like "Wessex Without Mycenae," about the impact of radiocarbon dating, the so-called radiocarbon revolution, which related to all the matters we've just been talking



about—changing chronological relationships and therefore explanations. David Collison, a director with the BBC's *Chronicle* program, contacted me and said he wanted to make a program about the tree-ring calibration of radiocarbon, which was the way many of these dates were being sorted out. I thought it was a very good idea, but the question was, where would we go on location to illustrate this? Well, as an undergraduate, I had already been to the wonderful temples of Malta. There were just a few radiocarbon dates for Malta, and it was clear that if you calibrated those, those temples had to be far earlier than any of the supposed Aegean influences—spirals were part of the story there too. So they would be free of Aegean influence, and you would have to pose the question, What on earth was going on?

So David Collison and I went to Malta, and we had a very splendid professional television journalist with us, namely Magnus Magnusson, who is indeed famous in England as a television person but is also a historian who has translated Icelandic sagas and so on. He was the person who presented many of *Chronicle's* programs. So we had lively discussions for the camera on Malta about the implications of all these things, and having done that, David Collison said, "Right. Now where might we go in Britain in order to exemplify the impact of this thinking?" It occurred to me that Orkney, with its spectacular megalithic monuments, was the place to go, though I'd never been there personally. So we went to Orkney, and we filmed on a number of sites, and I made predictions as to how it would look if the new



thinking was applied there. There were no radiocarbon dates available at that time from Orkney, but it was easy to predict how they might fall out, as it were.

In the course of my reading the literature on Orkney, it was clear there was one very spectacular tomb in a place called Quanterness, which had been entered in the last century and then had been left; nobody had looked at it again. So in addition to filming the famous site of Maes Howe, and the Ring of Brodgar, very beautiful places, we also went to call on a farmer, Mr. Scott H Marcus, at Quanterness Farm, to ask if we could organize an excavation on his site. That was the only permission required in Britain. I don't think that tomb was actually a scheduled monument because it hadn't been well known. In any case, that was the only formal permission one had to seek in detail, and Mr. H Marcus said yes. It did turn out to be a spectacular tomb, like some of the other Orcadian tombs. But none of them had been excavated in recent years, none of them had radiocarbon dating, and none of them had had their internal contents very carefully studied.

Once again, we did obtain good chronological materials, we recovered excellent radiocarbon samples, which, as predicted, put the Orcadian monuments early, in keeping with the other radiocarbon dates for northwest Europe, and far earlier than they had previously been thought to be. We were able to recover the human remains from within the tomb, and we were able to make estimates of how many people were buried there. We could begin to think about the relationship



between such tombs and their societies. This was, in a sense, a single-period site; though it was used for almost a thousand years, it was, in principle, one long phase of occupation, and there was a much later occupation, which we could look at separately. Essentially, the entire local community was buried there; it was a sort of equal access tomb—women as well as men, children, although not very young children, as well as adults. By looking at the distribution of these sites it was possible to begin to suggest what sort of role the monuments were playing in the society, and that approach has developed since and has continued to be a trend in British archaeology.

In terms of methods, certainly careful recovery, including sieving, allowed one to recover all the human remains and therefore to make suggestions about the population involved there. And the use of areal survey—when I say areal, I mean in the sense of area, not "from the air"—led to very clear conclusions. There have been many more excavations since which have reached more ambitious conclusions, but certainly one was working in a social context.

SMITH: Now, in your argument that the tombs served as boundaries—

RENFREW: As foci, really, not necessarily as boundaries. Territorial markers, I think, is the phrase I used.

SMITH: Yes, territorial markers. Is there a way of testing for the falsifiability of that proposal?

1. The first part of the document discusses the importance of maintaining accurate records of all transactions and activities. It emphasizes the need for transparency and accountability in financial reporting.

2. The second part of the document outlines the various methods and techniques used to collect and analyze data. It includes a detailed description of the experimental procedures and the statistical analysis performed.

3. The third part of the document presents the results of the study. It includes a series of tables and graphs that illustrate the findings of the research. The data shows a clear trend of increasing activity over time.

4. The fourth part of the document discusses the implications of the findings. It suggests that the results have significant implications for the field of study and may lead to further research in this area.

5. The fifth part of the document concludes the study. It summarizes the main findings and provides a final statement on the importance of the research.

RENFREW: It is quite difficult. The assertion that they are territorial markers doesn't necessarily suggest that they are marking the *edges* of territories; it may rather be the center, though very often the center would be the arable land, where people were actually acting out their lives and their residence. The settlements at that time were not always very clear, though they are becoming clearer now. So the tombs may sometimes be located close to the center, or they may be on the edge of the territory, but they would still be within the territory, and the argument is that you would have one major tomb for one major territory. It has proved difficult to test that, partly because you are not dealing with a homogeneous landscape; you're dealing with landscape with very major terrain differences. I showed maps where if you plotted out the tombs, they seemed to space across the landscape.

It has to be said that when people have tried to test these things in a formal way, using geographical techniques of location analysis, they haven't come out with any strong confirmation. I think it is actually quite complicated when the terrain is so mountainous; it's difficult to find very satisfactory formal tests. It's a similar problem when you look at the Iron Age in Orkney, where you have these different sites, called brochs, which are very numerous. It's almost self-evident that people were living in them as strongholds, and they were local centers, but to demonstrate that by a formal locational analysis hasn't really been done, and it would be quite difficult to do. So I agree it would be much nicer if you could come up with a very strong hypothesis, that



if these people were acting in such a way you would get certain predicted parameters in a locational analysis, and then you could do probabilistic tests to show that it's a significant result. I think, rather disappointingly, it hasn't worked out like that. I don't myself feel that to be a refutation, although some articles have been written saying, "Oh, this doesn't work out," claiming in effect to refute a hypothesis. I don't think people seriously feel that, because there are also other factors at work. Nonetheless, it cannot be claimed as a triumph of the hypothetico-deductive method, unfortunately.

SMITH: But you still have what you consider a reasonable interpretation that matches the information that you've got?

RENFREW: Yes, that's right, and although this doesn't prove it's right, I think most subsequent interpretations have been working broadly within that framework. Quite a few people write articles disagreeing in various ways, but in fact I think they are essentially working within the same broad framework, to ask what was going on in the society in local terms. There are very few people now talking about the megaliths of Orkney who are claiming that a bunch of settlers deriving ultimately from the Aegean had a landfall there, built Maes Howe, and then things went on from there. So I think we're all working within a different paradigm to that.

SMITH: In your textbook, you outline the major points that distinguish the New Archaeology from the old archaeology, and one of the points is that the testing of



hypotheses replaces the authority of the researcher. I wonder if that distinction had any relationship to your practical experience as a young person in the archaeological field?

RENFREW: No, I don't think it did to a great extent—but perhaps I was lucky. You were asking me earlier about how my own seniors in Cambridge responded, and as I indicated to you, they did not respond in an authoritarian way. Grahame Clark may have been a bit unenthusiastic about some things that I said, but that didn't change our professional relationship at all. He just rather disapproved of what I had said there; it was the same with Glyn Daniel, and indeed with others. For example, John Evans had presented very firm diffusionist arguments, as I would call them, saying in his initial formulations that the developments in Malta were the consequence of developments in the Aegean. We weren't working in relation to Malta when we were working in the Aegean at Saliagos, but that background in no way prevented our working very constructively on that project. So I don't feel at all personally that I've been subjected to much authoritarian hostility.

I think, indeed, the person who first gave expression to that view was Lewis Binford. I think it could be argued that he is not always the soul of tact. In his book, *An Archaeological Perspective*, which is one of his volumes of essays, linked together, he gives one amusing account of how he rebelled against the views of Jimmy [James B.] Griffin, who I think would be seen by Binford as one of the



authoritarian figures. He relates that one of Griffin's research students produced some important new sherds of pottery with unusual decoration. Griffin was one of the great scholars arranging cultures in their sequence, with enormous emphasis on the typology and the correct arrangement of forms and decoration, just the kind of thing that Binford was rebelling against and despising. A friend of Binford's, who was taking part in the discussion, seized hold of one of these sherds, cast it to the floor, and ground it underfoot with his heel, saying, "That's what I think of your negative decorated something-or-other ware!" This was in the presence of Griffin, who was clearly greatly displeased. So here was one of the expressions of rebellion against the traditional paradigm, but I don't think that kind of confrontation happened in a British environment. I think it may be that Binford and others did suffer; for a long time their work was thought to be inappropriate. Sometimes the coming of the New Archaeology, certainly by Binford, is presented as a sort of social revolution within American archaeology, which in many ways it probably was. I think it wasn't quite such a rebellion in Britain as it was in the United States.

I still think it's true though that sometimes statements are assumed correct if they come from "Professor So-and-So," as a person of enormous authority, whereas if they come from some research student, the response is, "Who's he? Never heard of him." I think Binford and others were right in saying that the respect for authority is part of a traditional approach to scholarship, particularly in the humanities. We were



mentioning earlier some distinguished figures in the field of classical archaeology. Well, if [John] Beazley attributed something to the Amasis Painter, you had to be very bold indeed to say, "No, not at all. It's by some other painter." There are these traditions of scholarly attribution which are matters of the finely-honed judgment, the "eye" of the experienced scholar. This I think is what Binford was rightly criticizing, whereas a scientific approach would be to say, "Let's look at the arguments. Let's look at the data." I agree with Binford's point, but in personal terms, because that's how your question was framed, I don't think I've suffered much.

There were snooty reviews. I remember Jacquetta Hawkes wrote a very dismissive review of the Saliagos publication for *Antiquity*. She said that reading some articles in recent archaeology was like "walking across shingle on a pebble beach." You know, sort of walking very hard and not getting very far. Well, one knows what she means, but she was using this to disparage a whole approach. I think she would be one of those who would be looking down her nose at the "new scientism," as it were, seeing it as an intrusion into the established "decencies" of scholarly intercourse. So I actually agree with Binford's point. She offered some observations on the Saliagos book which were very much putting me in my place. She was lamenting the day when scholars would write good English and handle our language appropriately, as did the great figures of the past, like Gordon Childe and so on.

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So one comes in for a little of that, but, equally, there's always a battle between the generations. To get back at Binford, it has been said that in order to make your mark, you have to assert that perhaps the modest statement you are making is a *radical* view which will transform our entire notion of science and will overthrow the traditional citadels, and therefore you have to assert that the old guard is obstinately manning the barricades of their citadels and preventing the new wine or the new brooms, whatever the image is, from being drunk or from sweeping clean. So there is something in the nature of a war between the generations which is often being dressed up as "the established scholars are so full of authority they will not hear these words of truth," or, "they will not recognize that the emperor has no clothes"—you can put it in a whole series of different images.

In this country I don't think we have suffered too badly from that. I have plenty of students who don't hesitate to say, "This is terrible nonsense. Renfrew got this completely wrong." I find that in student essays, and I don't look as sour about it as, say, many German professors do. The German system remains very authoritarian, and the New Archaeology has been suppressed there by an older generation of professors. I have had graduate students from Germany who have worked with me and done their graduate degree and said, "When I go back to Germany, I won't be able to talk like this. I'll have to work within the established paradigm." So there is no doubt that there are scholarly traditions where you really can't question the



wisdom that you have received from your seniors in that profession. Happily, in Britain, though there are often very sharp controversies, I don't think they are stifled by authority very often.

SMITH: In the United States one constantly needs to get letters from peers, and particularly when you are starting out, from seniors. Is that the case also in Britain?

RENFREW: Yes. If you are applying for a job you need referees, so you have to have letters from your seniors, and that can be a problem, but I don't think it's a major problem. Of course you don't always know what's said in the letters about you, but I'm confident, for instance, that Glyn Daniel, who was one of my referees, would usually write a very warm reference. I have no reason to doubt that. In Britain we have quite a lot of young professors of different views, and I don't think it's difficult for a graduate student looking for a job to get a letter from somebody in support of his case. Somebody could say of me at this point, "Oh well, he would say that, the Disney Professor in Cambridge; he's just upholding authoritarian views, saying the system is fine!" Exactly what we were talking about! [laughter] But I don't actually believe it works like that.

To give you an example, we have in this country quite a young group called the Theoretical Archaeology Group, which is now one of the major conferences in Britain every year. A friend of mine, Andrew Fleming, and I originally set it up about twenty years ago. Its keynote has always been that it's mainly graduate students, and



it remains a young conference. There are very few senior academics who bother to go there. Well, that has a youthful flavor to it, I think one can say, in a very healthy way, and I think it's very good fun. Organizations like that are quite the opposite of something like, say, the Society of Antiquaries—which I'm not criticizing. But it's much more senior; you have to be elected, you can be blackballed, you put the letters F.S.A. after your name, that kind of thing. That's the kind of society you could suggest is the image of authoritarian scholarship. But the Theoretical Archaeology Group is certainly the opposite, so I think the situation is quite healthy in Britain.

SMITH: I had wanted to ask you about Gordon Childe's position in British archaeology. I remember in my undergraduate days at Berkeley he was presented as Mr. Archaeology, at least for undergraduate education, and I gather from looking at your materials that he occupied a similar role in Britain, perhaps even more strongly. How did his Marxist perspectives interface with the spectrum of British archaeological thought? You mentioned that Clark was conservative, both politically and personally. Was Childe's authority despite his Marxism, or was there some way in which his British Marxism fit in with British Toryism?

RENFREW: I don't think it fitted in with British Toryism, particularly. There's no doubt that Gordon Childe was a very complicated person, intellectually. If he had much of a private life, it's never really emerged. There have been biographies written about him, and much of his life was his intellectual life. I think the answer to the



question, in a way, comes from the undoubted circumstance that there were two strands to his work. Much of his early work, and indeed his later work too, was of a very detailed character. It's not unrelated to what we were saying earlier about broad theories and specific culture sequences, and getting information together to allow one to comment on things. Childe's earliest work was in Greece; he was beginning to follow up language questions, but that didn't predominate altogether in his work.

Childe's first really thorough work was in the Balkans, which provided the material for his second book, *The Danube in Prehistory*. That book is just a massive compendium of information. It does indeed have a structure, but its contribution is really to get in amongst this material and sort it out. The first book which he published, *The Dawn of European Civilisation*, drew on some of that I believe, and, again, it was doing two things: it did survey the material in a very thorough way that had not been undertaken before, or not since an earlier generation, but it did nonetheless have a simple unifying principle, which Childe referred to at the end of his life as "the irradiation of European barbarism by Oriental civilization," which is one of the classic expressions of the diffusionist principle. It now turns out, I believe, that he was wrong about that. Some of his chronologies were wrong because they were based on those assumptions, but his treatment of the detail in each area was so thorough that the later editions of *The Dawn* are still very usable as standard works for the archaeology of each area. So Childe in his early days was really interested in

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getting to grips with the material—he did the same with his study of British prehistory—and that remains the contribution for which he's best remembered in this country.

Around 1930, he wrote his book *The Bronze Age*, which was beginning to apply Marxist thinking explicitly in a way which *The Dawn* doesn't do, because, you see, there's nothing Marxist about a diffusionist principle. If you think about Marxist thought, it would be more in harmony with processual archaeology. They're not the same thing, but Marx is looking at economic and social processes, so his approach could be described as processual. But I don't think processual archaeology is particularly Marxist, or it doesn't have to be; it was Marx, who was the pioneer of processual thought, and who then put his own spin on it with class struggle and contradictions. You don't have to think in those terms if you are thinking processually, but to emphasize the economy and social interaction and so on is almost what you mean by processual thought. Well, Childe then developed the concept of the neolithic revolution, which is still basically the way we think about that key transformation, and then the urban revolution, and he developed these ideas in his wonderful book, *Man Makes Himself*. I don't imagine any Americans read *The Dawn of European Civilisation*, and they certainly don't look at *The Danube in Prehistory*, but they do look at *Man Makes Himself*, and the sequel volume, *What Happened in History*, which was very much the same thing written a few years later, for Penguin.



Childe there was developing notions of how the urban revolution came about, and they were fundamentally processual writings.

If you are looking for the origins of processual thought—I don't mean so much the New Archaeology but of processual thought—you will indeed find in Britain work like Grahame Clark's *Prehistoric Europe, The Economic Basis*, where he's talking about processes. Julian Steward was also one of the great pioneers there. It's true that he was influenced by Marxist thought, but I think that's partly, as I say, because Marx was one of the first to develop what you would call a processual viewpoint. Childe was one of those who was talking about social and economic processes which led to urbanization. So it doesn't particularly help to say it's Marxist; it's Marxist only because Marx was a pioneer processual thinker. To say that you followed the thought of Marx has never been scandalous in Britain to the extent it has been in the United States. Only to a limited extent have we had anti-Marxist witch-hunts of the kind which were operating in the United States in the late or middle fifties.

So, yes, Childe was a Marxist thinker. What was notable was that he was able to maintain that in an entirely overt way. He was active in Marxist intellectual circles in Britain, he never concealed that, yet there is a paradox which people refer to: he was a member of the Athenaeum, which is the leading club in London's "club land." It still admits men only, wouldn't dream of allowing women to be members. These



London clubs are very traditional; some of them are for people who are not necessarily intellectuals, but they are still for moneyed people. The Athenaeum is a club of such a kind, but it has always been a club where bishops will join, and well-placed academics, as it were. Gordon Childe was not only a member of the Athenaeum, but he spent a lot of time there. An amusing story is told that when he would entertain friends for lunch there he would say, "Would you have a glass of beer?" and then he'd say, "I'll have the usual, please," and a tankard of champagne would be brought to him. I don't know if it's true, but he clearly enjoyed living well. It's also said that he used to *épater le bourgeois* by insisting on having *The Daily Worker* delivered to his table. Everybody else would be reading *The Times*, or maybe *The Daily Telegraph*, and there would be Gordon Childe ostentatiously reading *The Daily Worker*.

He was a paradoxical figure in many ways. So much of his archaeological work in Europe was written within a diffusionist paradigm, that even when he was trying to be rather revolutionary in his studies of the prehistory of Britain, he still followed a view which I myself would consider to be diffusionist. I don't think I've ever read any direct juxtaposition or confrontation of Marxist thought and diffusionist thought, but as I said earlier, there is nothing Marxist in the diffusionist principle, and so there was an internal contradiction in Childe's writing that has never been explored, between his processual thinking, which was encouraged and inspired by Marxism, and



the diffusionism. If you think about Marxist thought, about the internal dynamic of societies arising largely from economic and social conditions, there's nothing remotely diffusionist about that, and most of Childe's work was in the diffusionist mold, partly because he was always fascinated in the language question, the Indo-European question, which I believe was one of his motivating issues; it was his first work and he returned to it from time to time.

So Childe was a very interesting and complicated person, and you can't use him as an exemplification of one particular way of thinking, because he had several things going on at once, not all of which he adequately resolved. But he was the first processual writer in the field of archaeology, I think you could say that, in *Man Makes Himself*, and the other book, *What Happened in History*.

SMITH: To what degree is processual thought a continuation of a tradition in Britain going back to Herbert Spencer's rethinking of Darwin?

RENFREW: Well, this is where you as an intellectual historian may have a different sensibility from my own. I don't think I've ever met anybody who's read Herbert Spencer. I suppose if you speak to Eric Hobsbawm he probably spent his childhood reading Herbert Spencer, I don't know. I'm not sure whether some of the big intellectual ideas of the last century have had influence. I mean, I don't know anybody who's read Auguste Comte either.

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[Tape IV, Side One]

RENFREW: We were talking about Spencer, and I'm afraid I was being a bit skeptical. Only fairly recently have I ever got down to reading Spencer, and I must say, from a contemporary perspective, it looks very generalized stuff, whereas Darwin still does carry real clout, mainly because what he said harmonizes with what we understand of the mechanisms of genetic transmission, and indeed with work like that of Mendel it explains how it works statistically. Darwin still seems a really powerful force, and Spencer, really, these days, reads like very vacuous stuff. In relation to your question as to what influence there might be, I don't believe there was much in archaeology itself, partly because, again, the prevailing paradigm, the diffusionist approach, runs totally counter to an evolutionary approach, which presupposes that these processes are taking place anywhere, not just in favored localities.

I'm not so sure about the origins of the sort of evolutionary theory that developed in the United States with the work of [Marshall] Sahlins and [Elman] Service, for instance. It may have been influenced by Spencer and indirectly transmitted to us, but I'm not really aware of that influence. As I was saying, and that's why I was putting the ball in your court as a historian of ideas, maybe the whole climate of opinion in which we live, not just as archaeologists and anthropologists but as people in Western society, is profoundly influenced by Spencer, but I'm not aware that it is.



SMITH: If it is a Spencerian influence, it would naturally be in a textual unconscious, that is, the terms having been reworked and respecified so that you can deal with the particularities of the situation.

RENFREW: Right. On the other hand, my rather superficial reading of Spencer failed to reveal many particularities.

SMITH: On the contrary: his goal was to put things on such a general level that the templates could be fixed onto anything, and then you would see how to allocate your data.

RENFREW: Of course it's sometimes a sign that the influence of something has been greatly profound when you read it and you think, "That's nothing." You read Newton's laws of motion, which of course are today discussed more as axioms than full of predictive content, but nonetheless Newton so shaped the way we think about motion that his laws of motion are just obvious. It may be that Spencer's influence has been of commensurate power, but I think when you say it's of great generality, it is also true that if you explain absolutely everything you explain nothing, and so far I haven't been greatly seized by the points which Spencer brought to discussion. It may be that I need a good seminar from you or from somebody to show me how Spencer was a profound influence.

SMITH: Not necessarily a positive influence though.

RENFREW: But an influence, nonetheless.



SMITH: Well, at any rate, I was just throwing it out.

RENFREW: Well, I threw that one back! [laughter]

SMITH: However, there seems to be no question that Lewis Binford was an influence. Generally speaking, it's Binford's essay, "Archaeology as Anthropology" that's acknowledged to be the critical work that announces that something profoundly new is on the horizon. I wanted to ask you when you read it and how it affected you.

RENFREW: Right. To tell you the truth, I'm not quite sure when I did read it. I think probably it is the case that I didn't read it until I met him personally. It was a pleasant coincidence, really. I went to do a semester at UCLA in 1967, as a visiting professor, at the invitation of Marija Gimbutas, who was a leading figure there, although not at all of the same line of thought as Binford. She was mainly in the Indo-European studies department, but she also had a section teaching European archaeology and in that sense was in the anthropology department also. We met in 1965, when she came to visit the excavations at Saliagos. She was just beginning to take an interest in doing some fieldwork. I don't think she was a great fieldworker herself, but she was a great organizer and inspirer, and she wanted to get fieldwork going in Greece and the Balkans. She did indeed take part in, or initiate, a number of projects, in Thessaly, Yugoslavia, and particularly in Italy, and when she visited us in Saliagos I think she was clearly keen to get something going. So she invited me to spend a semester at UCLA, which I accepted and found a very interesting experience,



and it did indeed lead on to collaborative work at Sitagroi. Marija Gimbutas was fascinated by the symbolism of the European Copper Age, and we found many figurines which she published for our volume. I think she also had the departmental mission to create fieldwork opportunities for some of her students, which she did do.

The consequence of that invitation was that I met the faculty members at UCLA, which at that time was a very lively department, with Clement W. Meighan, an Americanist archaeologist, Jim Hill, who was one of the very bright new archaeologists, Jim Sackett, who was one of the leading American scholars in the paleolithic, all youngish people, and Lewis Binford was there. He wasn't at UCLA for very long. He'd moved I think from Santa Barbara, where he'd migrated from Michigan. I mentioned to you that he wasn't thought to be a harmonious presence there. Then he moved on to the University of New Mexico, where he really settled and did very good things in the department there. I'd have to reflect very carefully whether I knew much or anything about him before that time, because that was before he had published the book with Sally Binford, *New Perspectives in Archaeology*. This was their edited volume, which was the first book in the field of the New Archaeology. Certainly when I met him he gave me offprints, which I read with great interest. I discussed a number of things with him and found him very interesting and lively.

At that time I was interested in developing some seriation dating techniques

for the Cycladic cemeteries, so I was beginning to work on that with a graduate student there, Gene Sterud. We discussed this with Binford, who was splendidly scathing and had no time at all for seriation methods, because he thought this focus on dating artifacts by their relationships was just terribly old-fashioned and traditional. We had some very robust debates. From that time onwards I got on very well with Binford personally and he has been a friend since then. So that must be when I first read his papers. Although, now that I think of it, I had read a paper of his on agricultural origins the previous year. Somebody—Charles Higham, I think—had sent it to me when it was in typescript, so that must have been when I'd first heard of Binford. But anyway, I had the pleasure of meeting him then and getting to know him well, and we had some good discussions.

SMITH: Of course he developed what he was doing in relation to New World archaeology, which addresses some parallel questions, but largely a different set of problems, I suppose.

RENFREW: It's true that he was working in a New World context, but in fact the paper which I first read was on agricultural origins in the Old World. As he explained later, it was written partly to put right Professor [Robert] Braidwood, with whom he had done a joint seminar in Chicago, when he was a teaching assistant. So that paper related primarily to the ancient Near East. Binford's own interest has always been in hunter-gatherers, essentially in the paleolithic, and in earlier periods, so the New

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World doesn't have that much going for it in that respect. He was already deeply into issues of understanding Mousterian cultures, which are primarily in Europe but also the Near East. Much of his fieldwork has been to look at questions which could be relevant to the understanding of paleolithic hunter-gatherers. So, although some of his very early work was in New World archaeology, you wouldn't really regard him as a New World archaeologist, I don't think. Many of his articles, like your "Archaeology as Anthropology" reference, and another one on research design, claim, I think quite properly, to have relevance to archaeology anywhere.

SMITH: You said you had lively discussions. Maybe you could give me a sense of the aspects of what he was talking about that perhaps seemed questionable or dubious to you at the time, and perhaps since?

RENFREW: I don't really quarrel with much of what Binford says. His interests have always been primarily in hunter-gatherers, so he tends to make statements about archaeology which apply to hunter-gatherers and less so elsewhere. At that time, certainly, I was concerned with chronological problems, using artifacts for chronological purposes, and the whole thrust of his work was that all this space-time fixation, and the kind of charts of cultures and horizons that Jim Griffin was doing for North America was just the wrong way to use archaeology. He thought we should be asking real questions about processual issues. So, while I think that was a very reasonable thing to say, nonetheless, he himself wouldn't particularly deny that one

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had some responsibility to understand the time frame. It was on the virtues of seriation that we had some strong disagreements. Differences of view certainly emerged later, when he came to the Sheffield conference.

SMITH: That was in 1971.

RENFREW: Was it as late as that?

SMITH: Yes, if we're talking about the famous Sheffield conference.

RENFREW: That's right, yes, that's what we're talking about. Well, that was long after I'd been in California; it was Lew's first effective visit to Britain. He had been to London briefly before, but hadn't seen much of it. So that was another occasion when we really got talking, and that was also when he was very strong on the Mousterian issue. François Bordes was at the same conference. But later than that, when we were in Southampton, we invited him to spend some time there; it was virtually a semester that he spent with us, and he took classes and seminars and so on. He's a great man for rolling up his sleeves. He gave the students butchery classes, which rather staggered them—how you would butcher a sheep and so on.

He wasn't particularly well informed, nor did he seek to be, about the origins of complex societies, but he felt he could judge these issues from first principles, as it were, and so we had lively discussions there. I've never really disagreed with him, except that I do see some justice to the criticisms that have been made by colleagues like Ian Hodder. I don't necessarily agree with them that what Binford says isn't



appropriate or correct, but I think he does rather lose sight of the ideational dimension, and I think that's a very important dimension. I think it's partly because he's rooted in the archaeology of hunter-gatherers, when there isn't so much material that is available that is explicitly relevant to that dimension, so I think that rather shapes his outlook.

SMITH: In 1965 you take your position at Sheffield. How did that come about? Was it an open hiring and you applied for it?

RENFREW: It was an open, advertised job. I had come to the end of my research and had got my dissertation finished, and then I came back to Cambridge. As I mentioned to you, it was in Paris that we heard that I'd been offered a research fellowship at St. John's College, which I accepted. It must have been about a month later that the University of Sheffield advertised a position for a prehistoric archaeologist in the department of ancient history, so I thought about that. These days you wouldn't hesitate for a moment because jobs are so difficult to get. You would choose what was essentially a tenured position, a job that would keep going unless something went very wrong, not a research fellowship which would come to an end after three years. At that time jobs weren't quite so difficult to get, but I was of some doubt what to do, so I was interviewed in Sheffield. It was a very small department, with one other prehistorian, Warwick Bray, who was already there, and he was setting up a degree course in prehistory and archaeology. Administratively it

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was all under the direction of the professor of ancient history, a very stalwart doughty character, Robert Hopper.

I thought about it and sought Grahame Clark's advice, and he said he thought it would be all right either way. So my wife and I decided to take the rather prudent course and accept the Sheffield job. The people at St. John's were very nice about it. They said they wouldn't withdraw the research fellowship, it would become non-stipendiary, since I would have a stipend from Sheffield, but I could continue to have a room in St. John's. I proposed that I might go down perhaps once a week to undertake supervision and keep in touch in Cambridge, and Professor Hopper was willing for that to happen. So that was a rather pleasant way of seeing something of the life of a research fellow, which is rather a special situation to be in, although it in fact didn't give me all that much time to undertake research, since I was doing a full teaching program in Sheffield.

SMITH: What were you teaching in Sheffield, specifically?

RENFREW: Warwick Bray and I had to put together a complete degree course, which would take students over three years. There was archaeology combined with certain other subjects, but also single honors archaeology, which meant that students in their first year would do a range of subjects. They took introductory courses from us, and then in years two and three they would have courses from us only. So that was just two people doing that, and it wasn't really quite enough; it was quite a heavy



teaching load. But the plus side was that we were able to construct the courses as we wished, so that there were courses in archaeological science, and archaeological theory. It was mainly later European prehistory that we were teaching, with some paleolithic inserted. It was rather nice really; one was able to develop the course in line with one's own interests, and to continue developing it in that way. One was able to put what one thought ought to be in the course, and it was really very good fun.

Then, about two years later, Warwick Bray was offered a job at the Institute of Archaeology; it was and still is the only position in the archaeology of the Americas in Britain. So he accepted that position, and that left me as the senior prehistorian in Sheffield. A replacement was recruited for him, and then subsequently the department grew. Robert Hopper was a great academic politician, so the department underwent remarkably rapid growth. Within a few years we had five or six colleagues and were able to do all manner of courses. It was really great fun to be developing a department in that way. In retrospect it was probably very fortunate that Hopper sat on all the university committees, so one wasn't at all burdened by bureaucratic administrative work, which he really undertook. When it came to examination time we would have to engage in the bureaucracy of that, but Hopper fought all the battles on the faculty board and the senate and so on.

This was a time of course of expansion in archaeology, when it was becoming a majority rather than a minority subject. This was partly, I think, because of very

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successful television programs, like those that Glyn Daniel and Sir Mortimer Wheeler did, and indeed the long-term influence of people like Gordon Childe had, as we were saying, caught the public interest. So it was demand-led. There were plenty of applicants for archaeology, and in those days you would be able to admit them all and say, "Look, we need a new lecturer to do this." Robert Hopper led a very expansionist policy, and the department has continued to grow. Jane and I left in 1972, but it now has hundreds of students and is one of the largest undergraduate courses in the country.

SMITH: Did you also develop a master's or Ph.D. program?

RENFREW: No. We sought to have research students, but it was and remains quite difficult for some of the newer universities to recruit Ph.D. students. Sheffield does have many of them now, but at that time it was difficult. I remember we began to do some tree-ring work, and one person began to do a Ph.D. there in that, and indeed continued after I left and became a specialist in the subject. Masters courses we didn't do at that time, though they have started up subsequently there.

SMITH: Your wife is also an archaeologist. Was she planning on teaching?

RENFREW: She did indeed do so. When I went to UCLA for that semester, the department had to look around for a substitute teacher for one term, and she was the obvious person to invite to undertake the job, which she did do, and enjoyed very much. She was invited to continue, and we were joined by a further lecturer, Andrew

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Fleming and later by Paul Mellars, and the department grew and prospered.

SMITH: And this was at Sheffield?

RENFREW: This was in Sheffield, yes. So Jane was developing a successful career, and was appointed to full lecturer, but then when I was offered the job at the University of Southampton, we discussed it carefully. As so often happens when husband and wife decide to make the move together, if it's the husband who has been offered the new job, the wife has to surrender her existing job. By that time we had two children. With one child it seemed to work out having somebody in to look after our daughter while Jane was teaching, but with two it proved to be much more difficult to organize and administer, so Jane decided not to seek an academic position. She might well have got a position in Southampton had she sought one, but she didn't do so. She supervised many graduate students, and she continued to do teaching in her own field of paleoethnobotany as a visiting lecturer, but not as a full-time, established lecturer.

SMITH: I presume it was you who organized the Sheffield conference?

RENFREW: Oh yes. That was great fun. It arose in an interesting way. Peter Ucko had organized a conference on the domestication of plants and animals at the Institute of Archaeology in London which had been a great success. He brought in people from all over the place. He had developed a system, which I hadn't encountered before that time, of precirculating the papers, so that the conference was given over

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mainly to discussion, and that was very successful. And then he organized another one on urbanism, and I invited people to come up to Sheffield following the conference, and that went quite well. Then Peter Ucko invited me to take on the next conference in his series. He had a very good link with a splendid man, Colin Haycraft, who was the presiding genius and owner of Duckworth, which was a good publisher, and part of the deal was that they would publish the proceedings. Unfortunately Colin Haycraft died a few years ago. But that was a good way to set up a conference, ensuring it would be published, and this was an incentive to make this system work, whereby everybody that agreed to come had to submit a paper in advance. You're halfway to having a volume if you've got the papers in advance, even if they may need to be revised.

Other members of the department entered into the spirit of the enterprise, but, yes, I did organize that, and very exciting it was, because we had a lot of distinguished figures from different places and very good discussions. Edmund Leach came; he was a very lively strong-minded British anthropologist, who was skeptical about many aspects of archaeology. The plan to focus on discussion rather than reading papers, which was Peter Ucko's model, was followed, and it led to some really very good exchanges. It was hard work, but it was no great task to put the papers together quite rapidly into a volume, because we had most of them already, and it did make a good volume. We were talking about a lot of theoretical issues, and

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we'd structured it into sections that allowed these theoretical issues to be discussed. Lewis Binford later on said it was the first international conference in the field of the New Archaeology. It wasn't particularly calling itself that, but it was on the explanation of culture change, so it had a sufficiently theoretical bias, and it allowed many of the issues which the New Archaeology raised to come to the forefront. So it was really a very good and lively conference.

SMITH: Did you select who would come?

RENFREW: Yes, pretty much. I forget exactly how one did that, but I took a lot of advice and tried to attract people who would be interesting. We specifically wanted Lewis Binford to come, Kent Flannery came, and Bill [William L.] Rathje, who at that time was quite a young researcher who'd just had a paper in the *Proceedings of the Prehistoric Society*. So we got a good number of American archaeologists. It was the first time a lot of people, like Jim Hill and Jim Sackett, too, had given papers in this country, as far as I am aware.

We had quite a good contingent of French people, though a lot of them marched out in dissatisfaction. I never quite understood why, but they found that they weren't getting enough attention in the discussions and they were dissatisfied, so they took themselves off, which was a pity, but there it was. But François Bordes, who was French of course, wasn't one of those; he came and argued with Binford, and he argued some of the issues in the paleolithic. So it was really great fun,

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Professor Hopper, as usual, was supportive, and I think everybody enjoyed it. It was a very good enterprise.

SMITH: You mentioned [off-tape] that you brought philosophers and anthropologists, and I suppose sociologists as well, though I can't remember any.

RENFREW: Not so much sociologists. There are sociologists who are theorists of great interest. Years later, Ernest Gellner organized a seminar at the London School of Economics and he brought in sociologists with interests going beyond their own urban context in the twentieth century, people like Michael Mann, who was very good to know. But I have to say that many sociologists are busy analyzing suicide in Western society, or whatever. At that time the sociologists I had met didn't seem to have very wide-ranging intellectual interests.

SMITH: I was interested if there was work outside of prehistoric archaeology at that time that seemed to suggest new ways of doing the work that you were setting out to do.

RENFREW: When you say "work outside," do you mean within the field of archaeology, or outside the field of archaeology altogether?

SMITH: Outside the field of archaeology as well.

RENFREW: Well, this isn't really answering your question, but there has been the whole field of archaeological science, and we did earlier touch on some aspects of the application of the sciences. What one has to do as an archaeologist, if one wants to

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stimulate archaeological science, is see what scientific techniques might be used and then sit down and discuss them. To give you an example of that, as a research fellow of St. John's College, I found myself sitting next to Jim [James A.] Charles, another fellow whom I got to know, and he was a metallurgist. I told him that I had received some analyses of Aegean copper and bronze objects which had a very high arsenic content, and I couldn't understand why this should be. He became very interested and said, "Oh this is arsenic bronze, which has some properties very similar to tin bronze, and so it could be deliberate alloying." So we sat down and discussed that, he undertook analyses, and I wrote a paper on Cycladic metallurgy to which he added an appendix.

In a way, this is an example of how archaeological science emerges. It doesn't start as archaeological science, it starts as an archaeological problem, which some field of science can help. So this was metallurgy, really, but then if you mix the two together and use other terminologies we can call it archaeological science. There were many things like that. When we excavated at Sitagroi we wanted to understand how these great tell mounds formed. That was a problem in geomorphology, so we invited a geomorphologist, Donald [A.] Davidson, to come out there, and he became very interested in this, as did a number of others, and that's where the discipline of geoarchaeology was born, really. One of his colleagues edited a volume, and I suggested the right title would be "Geoarchaeology," and so you get these

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subdisciplines formulating. So that's the scientific field.

Then in the field of radiocarbon dating, there are many statistical problems, so it seemed appropriate to try and draw on the expertise of statisticians in the mathematical field. I did have a colleague in Sheffield, R. M. Clark, who ultimately did his doctoral dissertation on the appropriate statistical synthesis of radiocarbon data. But your question becomes a more interesting one when it's more than just matching specific techniques to archaeology. Simulation, as you know, is a technique that has been used by some historians, just as it's used by economists. When economists are trying to talk about time trajectories and forecast the behavior of economies, they use very complex simulations. So that's one approach which can be utilized. When I worked with Ken [K. L.] Cooke, who is a mathematician, on that edited volume, *Transformations: Mathematical Approaches to Culture Change*, we were deliberately bringing in as many people as we could who were thinking in mathematical ways about culture change.

So there's simulation, and time series analysis, and also catastrophe theory, which is broadly a topological approach. It is easy to slip over into mathematical mumbo jumbo, but there are ways of thinking about these things. Game theory has been very much used in evolutionary theory, mainly talking about competition among animal species, but it can be applied and has been applied in archaeological contexts. C. H. Waddington was an excellent mathematician who wrote the volume, *Tools for*



Thought, and I found that a wonderful book. It's one of the best books for broadening one's ways of thought, and I used to recommend it to all my students. You begin to get ideas of how trajectories operate through time and how you can begin to talk about these things. But I'm not insisting that we have to use mathematical language for doing these things. At the present moment, if one's interested in talking about cognitive aspects, one has to begin to work out how people are talking about the brain. Again, one can get rather bogged down in the mechanics of the brain, which don't really seem to help you very much with broad brain functioning, but it's still very interesting. I'm very fascinated by people doing work on consciousness; there one begins to look at work by philosophers like John Searle, who are focusing on particular areas. Then perhaps getting a little bit back to the hard sciences, at the moment the field of molecular genetics has something to offer in some areas of archaeology.

I don't really know that one can generalize about how ideas in other disciplines flow into archaeology. It's easiest when they are very specific things which just offer a method by which you can look at some specific archaeological problem, but there are also the larger issues about how you think. There's the whole field of the philosophy of science. There has been a lot of writing in archaeology about the philosophy of science, not always I think by the most distinguished philosophers, but that has helped to bring currents of thinking into archaeology. Then we were much



influenced by the neo-Marxist anthropologists. The whole neo-Marxist development in France and Germany had its impact on archaeology through one or two archaeologists who were very much interested. Archaeology is often very eclectic. My colleague here, Ian Hodder, who is one of the very vocal critics of Binford, has drawn very heavily on a whole range of postmodern thinkers; so there's a great wave of postmodernist thought washing over aspects of archaeology. Some of it to its benefit, some of it means you have to invest rather heavily in wading through other people's problems, really, before you come to much that is your own.

Perhaps the most obvious case of all is actually geography. The new geography was in some ways chronologically in advance of the New Archaeology, so that techniques of locational analysis were applied in archaeology, and to very good effect, since spatial analysis is well developed among geographers and continues to be so. Then the geographers, many of them, went into this subjective mode and had their own almost prepostmodern revolution. It was very subjective: what does it feel like, sense of place, and all that sort of thing, which has been picked up in archaeology. People are now writing books on the phenomenology of landscape and so on, which, if you had read the appropriate geography five years ago, you could have written then, to some extent.

SMITH: If we follow the geography connection, you did the work on predicting where center sites would be, locuses of power, which you test against the

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contemporary map of Europe—

RENFREW: Yes, how you might be able to predict what the political realities were by looking at the map of settlement, without any prior knowledge of power relations or of national boundaries and so on. That was interesting. We perhaps never exploited it as much as we should have done, because it was never picked up. We should have published it more in geographical journals. The geographers have their own ways of analyzing cities, but they've never really integrated the politics and the population figures as coherently as this particular model did. That study came about partly through working with a colleague with very good computer abilities, Eric Level. He and I conceived of this model, and he was full of enthusiasm as to how it could directly be applied in practice. We applied it first to the cities of Europe, just using population figures without any knowledge of where the political boundaries lay. If you vary the parameters somewhat, you obtain very different political maps of Europe. Some of them came very close to the modern situation, others gave you much larger political units, so that you had the boundaries of France extending almost to Russia, rather like Napoleonic Europe. But even more interesting, and nobody's picked this up, if you varied the slope of the line somewhat the other way, you actually found that the Union of Soviet Socialist Republics no longer held up; it fragmented into smaller states in pretty much the way it has done since. I've never emphasized that in conversation before, but, in a way, if you had predicted that some



parameters would change, you could then actually predict the map of the Commonwealth of Independent States.

SMITH: And then you apply it to Malta?

RENFREW: You're absolutely right, Malta, and also to early Mesopotamia. In Malta we had the temples and it was easy to look at that. Mesopotamia is an interesting case because Greg Johnson, using existing geographical approaches, had done very detailed analyses using the prehistoric settlement data available for the protoliterate period there. He created rather elaborate diagrams. He took a sort of hexagonal lattice, and by the time he'd modified it a few times it began to look like the pattern on the ground. I never felt that the rationale for modifying it as he did was totally convincing. We just used the sizes of the settlements as a notional monitor of population size and drew the maps, and very interesting it was.

SMITH: Did this prove to be useful in terms of looking for sites for excavation in some way?

RENFREW: I don't think it was intended to help you look for sites. What it was intended to do was to predict what the political organization and realities—in that sense the social realities—would have been if you have an input of survey data. In order to use it to predict site location or site sizes, you would have to have had prior knowledge of the political structure. It relates one to the other, so you'd need to have one as a known before you could predict the other.

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THE SECOND VOLUME

CONTAINING

THE HISTORY OF THE

REIGN OF

CHARLES THE FIRST

BY

JOHN BURNET

OF

THE UNIVERSITY OF OXFORD

[Tape V, Side One]

RENFREW: The model should predict for you, roughly speaking, which separate states are which separate states. I don't think it's necessary to predict precisely where the Great Wall of China is going to run, but it should give an indication of which are independent political entities. I don't think it's ever been tested in that way against what is known. It would be a very nice test of the model to take the Maya settlement data of a particular period without feeding in knowledge of political organization, and see how that works. I don't think that's been done, it would be a nice little project.

SMITH: In what practical ways have you used this technique in your own work?

RENFREW: Not to any very dramatic extent. I've certainly used it in speaking about the Maltese temples and in trying to think about the structure of Maltese society, and it did suggest that probably the Maltese temples were focal points of autonomous entities. It has been applied to Iron Age hill forts in Britain. We used this technique for the Orkney tombs, but that was more as a trial run, because we had the data readily available; we'd never really imagined that there was a very hierarchical society there at that period. I don't think I have applied it further, but it does seem to me a very basic problem, so that in the handbook of archaeology which I wrote with Paul Bahn [*Archaeology: Theories, Methods and Practice*], when we were discussing how you can examine early societies, it seemed to be something to put in early on. You do need to know how the power blocks operate and whether this is a



hierarchically structured society, and such an approach I think can be informative.

SMITH: The application of catastrophe theory to the sudden emergence and sudden collapse of states in the Cyclades I guess, or the Minoan—

RENFREW: Not in the Cyclades, particularly, but certainly in Minoan and Mycenaean civilizations, yes.

SMITH: Now, is the butterfly figure an effect of the calculations?

RENFREW: The butterfly catastrophe is the second in line. The cusp catastrophe is the simplest one, and that in some ways is the most informative. The butterfly catastrophe suggests a tripartite division, the notion of three states, as it were. But I think the most interesting insights come from the cusp catastrophe. It should be stressed that because catastrophe theory is a topological method, it doesn't actually generate numbers which give you predictions which you can test. It doesn't really give quantitative predictions anyway, or at any rate not unless you have knowledge of very complicated equations. The whole point of catastrophe theory is that when you look at certain phenomena you can say they must be governed by equations of a particular kind, and so you could expect certain behaviors without your knowing the equations. But the real point of it is to make clear in a very concrete way that sudden changes can have gradual causes.

I worked with Tim Poston, who is a mathematician who was interested in the applications of catastrophe theory to settlement pattern. When you sometimes get



very rapid shifts from a concentrated settlement pattern to a dispersed settlement pattern, it's often been assumed that this was because of sudden invasions or happenings on a dramatic and in that sense catastrophic scale. But catastrophe theory predicts rather neatly that when you just have gradual changes in productivity or population density, you can get very sudden effects out of these gradual changes. The same point can be made in relation to the sudden collapse of state societies. It's often been thought that if you have the sudden end of a state society, it must have some major disastrous cause: a major natural disaster, a significant climactic shift, or invasions of people. But, in fact, if you use the catastrophe theory approach, you can show how if you have just gradually changing features of decline, such as gradually adverse land exhaustion or something, the collapse can be a very rapid one.

SMITH: So you can provide an alternative explanation. Do you then proceed from there to suggest that that explanation is in fact more parsimonious and more effective?

RENFREW: There you have a real problem. Catastrophe theory will show how you can indeed have gradual causes produce sudden effects, and make this entirely reasonable, and in some senses entirely plausible; it's been applied, for instance, to stock market collapse, to the heartbeat. Christopher [E. C.] Zeeman has worked on many applications and shown how the heartbeat can be modeled in this way. But because it's not predictive in its specifics, I think it's only broadly of heuristic use. In other words, it's just as reasonable to suppose that this sudden change in settlement

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pattern or a sudden decline, or indeed sudden emergence of something, works upwards as well as downwards. That's why I introduced the word *anastrophe*—it's the opposite of *catastrophe*—for something that is a sudden rise rather than a decline.

So catastrophe theory shows how these things can happen, but, unfortunately, it doesn't really allow you to model in a more specific way to show how or when these things will happen. It doesn't really allow you to say whether that approach is the right one in a particular case. It just gives you an alternative framework, and that is the reason I think it hasn't really been followed up very much, or indeed proved very useful. It gives you a nice perspective, a nice way of thinking about the problem, and I think that is very helpful, but that being said there isn't a great deal you can do with it in archaeology, so far as I am aware. I haven't kept up with Christopher Zeeman's work, but I don't think it has revolutionized thought in the social science fields.

SMITH: At the same time you were engaged in this work, [Luca] Cavalli-Sforza was publishing his first work on the wave of advance model.

RENFREW: Yes, with Albert Ammerman at that time, that's right.

SMITH: Were you involved with that in any way? Was there communication between you and them?

RENFREW: Absolutely not, I didn't know either of them. They published a short article about it, I think, in the periodical *Man*. I don't remember where their very first



paper was, but I read that and I thought it was really interesting, and a neat application of mathematical modeling of quite a specific kind to a problem. One could see how it was a very plausible model for the spread of agriculture. I invited them to take part in the Sheffield conference we were speaking of. I didn't meet Cavalli-Sforza at that time, but Albert Ammerman came to the conference, and the paper they then produced in the conference volume is very often regarded as one of the standard early statements of that model. Much later they published a book where they said more about it, but, actually, it's such a neat, simple idea that you can express it in a paper. There isn't all that much more to say about it. Later on people said, "Look, it doesn't work terribly well in detail." That's perfectly true, because it's talking about a homogeneous plane in an abstract situation, and when you introduce second-order complications, it's not at all surprising that one finds that real life isn't that easy. But I still think it was a very elegant paper, one of the best applications of a mathematical model in archaeology.

I met Luca Cavalli-Sforza much later. Earlier on I wasn't much interested in genetics in archaeology because I had just seen all this terrible nonsense about craniometry and skull measurements and a tangled muddle I thought that was. I used to look very carefully at [J.] Lawrence Angel's work when I began my work in the Aegean, and I just thought that this was casting no useful light on the prehistoric past whatever. It's different when you are dealing with paleolithic times, when you're

[The text on this page is extremely faint and illegible. It appears to be a multi-paragraph document with several lines of text per paragraph.]

looking at very major changes in morphology. I'm not denying that people studying fossil hominids are doing great work, but I did form the view that people comparing "gracile" heads and "Nordic" and "Mediterranean" types was really useless stuff. Again, I'm not criticizing the pathological work: aging, sexing skeletons, seeing what people died of, nutritional work; I think that's all very interesting. But the notion of suggesting hereditary or genetic affinities through a gross morphology of skeletal remains, in particular skulls, seemed to me not good. I think there may have been more sophisticated statistical works since, so maybe that condemnation doesn't apply so well now, but it really did twenty years ago.

It wasn't until molecular biology began to be applied, over the past ten years, that the situation became interesting. I met Luca Cavalli-Sforza about ten years ago, when he was in Cambridge, I think at the invitation of Anthony Edwards, who had worked with him. I think Anthony Edwards perhaps suggested I should meet Cavalli-Sforza, so I invited him to lunch in St. John's, where I then was, and we've kept in touch since. His work is very interesting. Of course that article you were speaking of had nothing to do with the application of genetics at all, it was the spread of agriculture, as you know, and it's only more recently that Cavalli-Sforza has actually been applying human molecular genetics to archaeology, to very interesting effect.

SMITH: I was wondering who provided the community of inquiry that you were part of. Who were the other people that you could talk to regularly—which doesn't

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necessarily mean every day—about these experiments in providing alternative ways of looking at archaeological materials and explanatory processes?

RENFREW: It's an interesting question, and I think it's probably fair to say that the community was primarily an international one. It's partly an institutional question, in a way, as to what were the opportunities for the exchange of ideas. At the time we're speaking of, 1965 or thereabouts, the Society of Antiquaries of course was totally conventional, and the Prehistoric Society papers were generally more factual than theoretical, although the Prehistoric Society had an annual conference which often threw up more exciting things to talk about. At that time I think most discussions were more between specialists in particular subject areas. I think if we're talking about communities, then probably the ways in which archaeologists actually meet are quite important. I was mentioning the conferences organized by Peter Ucko, which I think were a great start, and our Sheffield conference that I was speaking of as well. I was always quite impressed, less so more recently, by the annual meeting of the Society for American Archaeology, the SAA. At that time the discussions were much more open to theoretical argumentation, and I used to go to the annual meetings.

I remember one year at the Philadelphia SAA, it might have been '74 or '75, I organized a session and invited a lot of British archaeologists, and that worked very well. I had always admired the way that the better established universities would have their own party during these meetings: you'd have the Michigan party, the UCLA



party, or the Tucson party. So I decided we would organize the British party, and it proved to be a memorably successful function; it's still spoken of in the annals of the Society for American Archaeology. I asked each person coming to buy a bottle of malt whiskey at the duty free, so we started off with fifteen or twenty bottles of malt whiskey, and then we went down to the liquor store, intending to buy quantities of beer, and the owner called the security guards; he thought there were six very suspicious people here about to loot his liquor store, but it was our intention to pay, and indeed we did. The hotel was very helpful, because none of our rooms would be big enough to hold much of a party, so they actually let us have the presidential suite. The party was a great success and was talked about rather more than our intellectual session. I mention that because the SAA has grown and has suffered now from the rise of salvage archaeology in the United States, so the meetings are partly exchanges of information for professionals working in the salvage field, which has diminished the proportion of theoretical input.

It was partly in the image of successful SAA meetings that Andrew Fleming and I organized the Theoretical Archaeology Group, which I mentioned earlier, because that certainly is now a forum where people exchange rather half-formed, and sometimes half-baked theoretical ideas. These were people who were interested in talking about ideas; they were the younger academics, really. At that time, when it wasn't so difficult to get a job, if you were seriously interested and doing quite good

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work, then you might get an academic position somewhere up or down the country. So it was some of the younger university lecturers in archaeology. I'm not saying it was only university lecturers, there might have been one or two from museums. I say some, because there were also those who weren't in the least interested in theory and just wanted to dig their trenches and catalog their scarabs, or whatever. Until some of these conferences were set up, there wasn't an institutional means by which they met very regularly.

SMITH: Given your work in the Aegean, you had some overlap with classical archaeology; there has been a movement of New Archaeology within that field, and it's perhaps more controversial than in prehistoric archaeology.

RENFREW: Is it more controversial? It's certainly been belated; it took a long while to get going.

SMITH: Yes. I was wondering what sort of connections you had with classical archaeologists and how you perceive that development.

RENFREW: Well, it's fair to say that initially most classical archaeologists regarded some of these tendencies with suspicion and disdain, and indeed that is to some extent still the case. I went to the Mycenaean Seminar just a week ago, which is a seminar in London which was formed more than twenty-five years ago, just at the time that Michael Ventris was deciphering Minoan Linear B. The original focus was in decipherment issues, but since then it's broadened out very much into the whole field



of Aegean bronze age archaeology. It still runs at a very high standard, but it is a mixture of all the prehistoric Aegeanists, and many of them are of classical formation. Professor George [L.] Huxley, who is a classicist who has worked in the prehistoric field and is very knowledgeable about much of it, delivered a great diatribe at the beginning of the meeting, saying that he was scandalized by the current antidiffusionist tendencies and this was a fad which has now seen its dying day. I assumed he was partly attacking me, but, interestingly, he chose to target mainly Dr. Oliver [O. T. P. K.] Dickinson, who has written a very good survey of the bronze age of Greece using processual ideas and handling the data very efficiently and very expertly. So Huxley dismissed this approach; he was talking about the origins of Greek speech in Greece and came up with some rather odd theories that it all came from Persia, really. Dr. Dickinson, who was present, had no difficulty in offering a very robust riposte.

So there's a curious mixture there. I remember, more than a decade ago, the Archaeological Institute of America asked me to take part in their centenary celebrations. As I'm sure you're aware, although it calls itself the Archaeological Institute of America, it's really the organization that has devoted itself primarily to classical archaeology, and also Egyptology. It had debates in its early years and agreed it would be interested in the archaeology of the Americas, but in fact it has completely steered clear of those issues, which is how the Society for American



Archaeology grew and prospered. Americanists go to the SAA, whereas Aegeanists go to the AIA, although a lot of people do now go to both meetings. So I was invited to give a paper, and did do so, and the subtitle was "The Great Divide," which referred to the gap between the thinking of the classical archaeologists and the prehistoric archaeologists. This was more than ten years ago that I was advocating the application of some of these modes of thought.

But I think what has happened hasn't been so much due to the influence of the New Archaeology as such, but rather, there have been a few individuals who have thought for themselves in a similar and perhaps in a parallel way. Anthony Snodgrass is perhaps the leading figure here. He's professor of classical archaeology in Cambridge, but he was a lecturer in Edinburgh when he did a lot of his early work. He's written on the dark ages of Greece in a way that makes great sense to many of us. There have been a number of very good scholars here in Cambridge, actually, who I'm sure were benefiting from the fact that in classical archaeology there was a line of thought that was well disposed toward such work, and there was a climate of talking about archaeological theory: Ian Morris at Stanford, for example, is one of the leading exponents of such approaches; he now works in the United States.

I suppose the American equivalent, really, would be those people who were working in the field of prehistoric archaeology but had a classical background. I think prehistoric archaeology has led the way: until recently, if they were digging a site of

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the classical period, they wouldn't have dreamed of collecting the animal bones or the plant remains; they are just getting there, just beginning to get pioneering articles where they've said, "Look, we've got some animal bones and we'll tell you what we've found, and we're actually beginning to think of studying nutrition in the classical period on the basis of the plant and animal remains." In Cyprus that's been going on for a while because Professor Vassos Karageorghis is more forward looking, but at many classical excavations you still wouldn't do that. So I think to some extent such approaches have filtered through partly from prehistoric archaeology, and perhaps partly from the influence of the *Annales* school of French history, which has also had an impact.

SMITH: Did *Annales* have an impact in shaping your thinking about trade issues and your use of econometrics?

RENFREW: I really wasn't very aware of it. When [Fernand] Braudel's book *The Mediterranean and the Mediterranean World in the Age of Philip II* came out, I read it with great admiration. I thought it was handling the data in a very good way, mainly in the way that an archaeologist would be handling it, and it would be very interesting to see it applied to the pre-medieval period. But I don't think the *Annales* school has had very much impact, even in French archaeology, partly because the French and the German archaeologists have been much slower than the Scandinavian, Dutch, and British archaeologists to adopt a processual approach.



SMITH: In the article "Trade and Culture Process [in European Prehistory]", you talk about the myths of trade routes, but it also seems to me that you begin to problematize the concept of trade. You want to remove it from traditional market economy stereotypes. How did your thinking evolve along those terms? Were there readings that you were doing that were suggesting different ways of looking at economics?

RENFREW: Yes. First of all, it's true that I tried to start just from looking at what was going on. What you find in the archaeological record are objects which can be shown to have traveled a long way, and in many cases you are entitled to assume that they were traded. What you very rarely find documented is the trade *relationship*. You may find obsidian over here, but what was being traded against it is not clear, so you don't get a very full picture of trade relationships. So it's right to face that, but I did begin to think in a more abstract way of trade as action at a distance. I was deliberately trying to think about how what happened in one place might be affecting in some way what happened in another place. So, that, in a way, is looking at issues which diffusionists might have looked at, completely avoiding the pro- or anti-diffusion argument, but just regarding trade as one mode of action at a distance.

It is true though that the school of Karl Polanyi was influential there, because Polanyi was taking an approach which was very much against the formalisms of economics, and developing what he called a substantivist approach—what really



happened? It was Polanyi who argued that market exchange was a very late development, and he laid emphasis on reciprocity and redistribution. These were very helpful ideas, though more recently Polanyi has been criticized, quite plausibly, by a number of archaeologists, most significantly Robert Adams, who has suggested that the profit motive and effective market economy—the entrepreneurial aspects of trade—must have existed much earlier. I'm sure that's right, but Polanyi very usefully threw out the window, for a while anyway, some of the concepts of classical economics; to use all these parameters doesn't make much sense unless you are actually imagining that various market processes are working. To use such terms in a nonmonetary economy, for instance, is really quite difficult, I think. So Polanyi's writings were very helpful and although in some ways they have been outdated, in other ways they have led on to good work. George Dalton, for instance, who is an economic anthropologist, has laid great emphasis on notions of primitive valuables, picking up ideas by [Bronislaw] Malinowski, who spoke of spheres of exchange, and this is all very interesting and very useful because it's putting exchange in a social context and really looking at the economy as embedded in society.

SMITH: I think we have discussed the general overall framework for your article on Varna, "The Autonomy of the South-East European Copper Age," but—

RENFREW: Just to correct you, if I may, Varna didn't come into that article. You are absolutely right, Varna is in Bulgaria and is of that period, but it was in fact

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discovered after that article was written. I won't claim that it was a fulfillment or a testing of a hypothesis, because Varna brought altogether unexpected things to light. On the other hand, it did document one thing: I had argued in that article that copper metallurgy was independently invented in southeast Europe, and that was pooh-poohed by a lot of people, but when it turned out that the earliest substantial finds of gold were at Varna, that had to mean, unless more evidence came to light, that this was the first place that gold was worked, and somehow in a general sense that lends support to the notion that this is the place where metallurgy developed early. So I think that article persuaded a lot of people that there was something to this notion of independent metallurgical origins in that area.

SMITH: I did note that this article apparently was one of your more controversial ones; perhaps that's what you meant when you said it was "pooh-poohed."

RENFREW: Well, it has been criticized. Theodore [A.] Wertheim had written interestingly on the origins of metallurgy, and had rather tended to assert that this was something that only happened in one place. He'd got a very fine ringing assertion of that principle, which I've quite often quoted in order to criticize it. And certainly Jim [James D.] Muhly, who is not a metallurgist but has specialized in questions of early metal working, on one occasion was very rude about this and said my writing in this manner was altogether doctrinaire, but I've never really understood why. It was making a new assertion, and some of the people in southeast Europe were rather



astonished by it to start with, I think, but it's been very largely accepted, and I don't regard it as all that controversial. I mean it was new, and it was part of what changed thinking about southeast Europe.

In one respect it was controversial. Professor Vladimir Milojčić was in his way the great specialist on the neolithic of southeast Europe, and in 1949 he published his major work on the chronology of southeast Europe. That was the year when [Willard F.] Libby produced his first radiocarbon dates, although not yet for that area. About the time I published that paper I went to the international conference in Belgrade; it was probably in the early seventies. I gave my paper there, talking about independent metallurgical origins, and there it was certainly controversial; indeed, I remember very clearly what I thought was a very strange incident. We were assigned I think twenty minutes to give a paper, and then there was about five minutes discussion time. I'd carefully selected about fifteen slides with which to illustrate my paper, and I made what turned out to be a tactical mistake. I had made available the synopsis of my paper the previous evening. I hadn't met Professor Vladimir Milojčić, but he was a major figure, and I was a very junior lecturer. He worked in Germany, though he was of Yugoslav origin, and in the Germanic tradition, one didn't disagree with so eminent a person. He stood up at the end of my lecture and said I couldn't possibly be right. He spoke politely enough, but attacked my work very firmly and showed about twenty slides by way of refutation. He took

The first part of the paper discusses the importance of the study and the objectives of the research. It then proceeds to a literature review, followed by a description of the methodology used in the study. The results of the study are presented in the next section, followed by a discussion of the findings and their implications. The paper concludes with a summary of the main points and a list of references.

The study was conducted in a laboratory setting, using a series of experiments to measure the effect of different factors on the rate of reaction. The results show that the rate of reaction increases with increasing temperature and concentration, and decreases with increasing volume. The findings of the study have important implications for the understanding of chemical reactions and the design of industrial processes.

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about twenty-five minutes to refute my twenty-minute paper, which I thought was a degree of overkill.

So we did have a set-to. He I think was speaking in German, and at that time my German was fairly fluent, so I replied to him firmly in German, and then I remember there was some other session where a paper was in French, and we disagreed vigorously in French then, so it's perfectly true that I really did cross swords with Milošević on that issue. He was one of those few people who completely dismissed the entire validity of radiocarbon dating, and he dismissed it quite early on, so that when tree-ring calibration came in, "I told you so" was his view of the matter, whereas in fact, the calibration put a number of things right and set radiocarbon dating on a much more accurate basis. But there I'm afraid he was just sticking to his diffusionist principles: it was very much that "Vinča follows Troy," following in Childe's footsteps, and there I'm afraid the radiocarbon dating and the evidence of Sitagroi just show that that can't possibly make sense.

SMITH: I had a question about your article, "Neolithic Trade Routes Realigned by Oxygen Isotope Analyses," the one about the *Spondylus* shells. You note that you were surprised at the result that the shells came from the Aegean rather than the Black Sea.

RENFREW: To some extent I was surprised. It does turn out that there are no *Spondylus* shells in the Black Sea, nor as far as one can determine, have there ever

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FROM THE FIRST SETTLEMENT
TO THE PRESENT TIME
BY
JOSEPH NEALE
OF THE BOSTON BAR
IN TWO VOLUMES
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been, so perhaps one shouldn't have been too surprised. On the other hand, when I visited Bulgaria and Romania for the first time and saw these great workshops of *Spondylus* shells, I had assumed that the species was one that was native to the Black Sea, and indeed that had been assumed by many archaeologists.

Again, it's a question of characterization: when we showed that the analyses of Melian obsidian matched up very closely with the analyses of paleolithic obsidian in the Franchthi Cave, a number of people said it couldn't possibly be so, there must be closer sources of obsidian, because they couldn't imagine that people were seafaring at so early a date. So they assumed there were unknown obsidian sources which we hadn't yet found, so they started rushing over Methana, which is a volcanic peninsula, trying to find obsidian and very confident they would do so, but they didn't find any obsidian there.

But it had often been assumed that the *Spondylus* trade was indeed up from the Black Sea, because if not, where was it coming from? Well, it must have been coming from the Aegean, but at the time there had been no major sites in the north Aegean found with any *Spondylus* shells in them. From Sitagroi, and also from Dikili Tash, the nearby site, we have quite abundant finds, so it's clear that the trade route must have been up from the Aegean. *Spondylus* was traded very extensively right up the Danube, as far as Austria, so it was a very long distance early trade. It remains of great interest, and it is quite important to know where it came from. Given the high



frequency of *Spondylus* in the Gumelnița culture of Bulgaria and Romania, it still remains to me mildly surprising that it wasn't coming from the Black Sea. I think most conchologists would be unsurprised to be told that prehistoric *Spondylus* wasn't coming from the Black Sea. They'd say, "Oh, we never thought it was anyway." But I think it's useful to be sure of that.

SMITH: I can understand the degree of tension generated by the issues that you are raising by senior figures who had developed life careers based on a certain kind of argument, but it seems the heat goes beyond that.

RENFREW: Does it? Certainly, you can say Professor Stuart Piggott managed to remain perfectly polite, though not always enthusiastic. He published his standard work, *Neolithic Cultures of the British Isles*, in 1954, I think, and certainly it was and remains a very fine book, but in those days one of the end products of such a book was the chronology, and I think it was irritating to him when his chronology was all proved wrong. It wasn't just my argumentation; it was the radiocarbon dates which proved him wrong. In the case of the Stonehenge story, I think Richard Atkinson was irritated that his great theory that it really was of Mycenaean origin was questioned. That was more argumentation in the beginning, although later the radiocarbon dates came though very clearly. I don't think it's surprising that these people were a little put out. But they remained civil enough. So I'm not sure there was terrific heat.



SMITH: It seems like there was a lot of argument about this issue, which continues even to this day.

RENFREW: Well, George Huxley at the Mycenaean Seminar was really very rude to Oliver Dickinson, and fortunately he was there and was rude straight back.

SMITH: It struck me, particularly looking at *Current Anthropology* and the tenor of the replies, for instance, on the Indo-European—

RENFREW: Ah right, but that's another issue.

SMITH: It's another issue, but somehow or other, the old insults seem to get stirred up.

RENFREW: I mentioned earlier that Saul Weinberg had made available to me that surface collection from Mykonos in the American School of Classical Studies at Athens, which I had written an article on. Theoretically, the article was done with John Belmont; he didn't have any hand in writing it, but he originally collected the material, so we made it a joint article, and I was very happy with that. Saul Weinberg was very nice and encouraging at that time. But when I published my book, *Before Civilisation*, he wrote a really quite vituperative review of it, perhaps in *American Anthropologist*, I don't actually remember where, to extent that they wrote to me saying they felt that I should have the right of reply, which of course is not usual in book reviews. I did avail myself of the right of reply and had to speak fairly crisply back, so if you wanted to find fairly firm exchanges of views, that would be a good

[The text on this page is extremely faint and illegible. It appears to be a list or index of items, possibly names of people or places, arranged in several columns. Some words are difficult to discern but may include terms like "John", "Mary", "James", "Elizabeth", "Thomas", "Richard", "Henry", "William", "Robert", "John", "Mary", "James", "Elizabeth", "Thomas", "Richard", "Henry", "William", "Robert".]

example of one. In fact, Weinberg emphasized that I was flogging a dead horse. I pointed out that although on the one hand he was trying to suggest that what I said was already well known and didn't need saying, on the other hand he was contradicting it, and that Weinberg's dead horse was trying to run with the hare as well as with the hounds; so that was the level, I'm afraid, of the exchange.

[Tape V, Side Two]

SMITH: Ian Morris has suggested that part of the vituperativeness is due to a hundred plus years of cultured erudition and identity for the educated elites in Europe and America, and of course that seems to me an unfalsifiable speculation.

RENFREW: I'm not sure it's unfalsifiable. First of all, I don't imagine that's Ian Morris's own view; I'm sure he's accounting for it, but I don't imagine he is objecting to my writings on those grounds.

SMITH: Oh no, not at all, he's explaining—

RENFREW: But you see, funnily enough I've also been criticized in some quarters for being rather chauvinistically pro-European. My book *The Emergence of Civilisation*, which is the one on Aegean prehistory, was suggesting that these civilizations came about in Crete and Mycenae, and it was rejecting the notion that they came from the Near East. Likewise, the Indo-European argument as applied to Greece: I'm suggesting that the Greeks were "autochthonous," i.e., that the Indo-European speech came in much earlier than had generally been supposed, and that the



Greek language therefore formed in Greece. Now, this annoys a lot of Hellenists, it's against what they have long believed to be the case. Indeed, very few Greeks believe it; it annoys many of them too, because some of them have spent years working out which parts of the Ukraine the Greeks came from, so they are dissatisfied at being told there's no truth in that theory at all, which is my view. But, nonetheless, I remember when I excavated at Saliagos I originally had some of these ideas, and in Kathemerini I gave an interview, and the heading of the newspaper article was "The Greeks were Autochthonous." Well, you can't accuse me of undermining pure Hellenism by such a simple idea, can you?

SMITH: No, but British Hellenism, or German Hellenism then traces Western civilization back to these roots, so that there's a sense of continuity. It's hard for me to see why an indigenous British culture couldn't be as much a source of pride or romantic affiliation as the idea that somehow everything wonderful from Britain came directly from Greece for the last three thousand years. But, nonetheless, I think that is what Morris is suggesting, this idea of a common source, which then makes European culture unified and unique.

RENFREW: Yes, you're quite right. Gordon Childe, for instance used the phrase "irradiation of European barbarism by oriental civilization," but Mycenae was for him the secondary center, so that also implied the irradiation of European barbarism by Mycenaean civilization, that's quite right.



SMITH: *Before Civilisation* strikes me as a book that's been directed primarily to an educated popular audience; is that correct?

RENFREW: To some extent. I've never really made the distinction very much in writing a book. Most of the books I've written have really been written for myself, in a way. I had made two or three broadcasts on the *Third Program*, which in those days was a serious radio program in this country, and the broadcasts were published in *The Listener*, which at that time was quite a high grade journal, based on what was broadcast. There were a surprising number of inquiries from publishers, and they said they'd like me to write a book on this. So I did in the end get signed up with one publisher, Jonathan Cape, and they were keen to have a book that would be intelligible. My main concern, which was also true with *Archaeology and Language*, was to be sure that it would be coherent to archaeologists who were interested. I had some debates and arguments with them about the referencing system—they of course didn't want footnotes. That didn't worry me so long as all the relevant material was gathered up at the back of the book; there are endnotes, and there's a perfectly good bibliography, so it wasn't really very different from writing serious articles. They employed a professional editor, who sat down with me in a way which at the time was tiresome, but ultimately it was very helpful in pointing out what wasn't clear and where I'd repeated myself. She was just taking it from the standpoint of a reader who didn't know much about it, and she certainly helped to clarify it without our



disagreeing too much and without losing anything of any great significance.

SMITH: At the same time you started to do your BBC television programs, so it seems to me you're starting to think about how to present this material to a larger audience.

RENFREW: In a sense yes, except that was more the case when I was writing the radio talks. I had to know what I was going to talk about and how I was going to say it. But you don't do a television program in that way. You are invited to do a radio talk, and maybe nobody's even read the script before you give it, whereas, generally, if you are invited to do a television program, there is a director, a presenter and so on. So I was invited to do a program, and we then set out to develop what the story would be and how it would be structured. For the early programs it was Magnus Magnusson who wrote the linking passages. They certainly asked me if I was happy with them, and I would change some things. It was three or four programs later that I was invited to be the presenter and also to write the narrative passages as well as the direct to camera passages. So I think with a television program it's still very much the director who controls the shape; he is asking himself how we are going to present these ideas, and which aspects of these ideas we are going to select. It wasn't really a deliberate project on my behalf to promote certain ideas; I was simply being invited to do a program on this or that, and then I would suggest what might be interesting. It's true that one's own interests and enthusiasm would help to determine the direction of



the program, but one wouldn't be saying, "This is how it will be."

SMITH: I have some general questions that have to do with the culture changes that were occurring in Britain during your youth and early adulthood. One of the other people we've interviewed who was at Oxford mentioned that the generation that was born in the thirties was dedicated to revisionism in every aspect of life. Of course, that's a generalization that immediately has to be qualified and be suspect, but there was the sense of a generation that was separating from older ways of looking at identity and the relationship of Britain to the rest of the world, and the senses of hierarchy within Britain. Would you agree with that?

RENFREW: First of all, I'm not very sure what he means. Secondly, born in the thirties, I wouldn't have quite thought of it in those terms. Anybody born in the thirties wouldn't have taken part in the war if it began in 1939, so really, "born in the thirties" is a way of saying those who became adults after the war.

SMITH: Or in the fifties, yes.

RENFREW: Or in the fifties. Yes, that's right. If you look at what was happening in England, I don't think there was a particularly revisionist spirit until much later than that. Indeed, I think Britain could sadly be criticized for failing to notice that times had changed. Politically, Britain went on claiming to be a major power until well into the fifties, when it could no longer afford to do so. And this wasn't just Churchill, although that might be expected. Clement Attlee of course introduced many changes,

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but the defense budgets were huge after the war in a way that wasn't warranted. It should be said that the Festival of Britain, in 1951, was a great occasion for celebration, a great time of national self-confidence. I don't remember exactly when it went into decline, but in the postwar years the Commonwealth was still seen as a great force, and then suddenly all of that was understood to be almost entirely irrelevant. Although the Commonwealth as we look at it today has a historical background that is a pleasant and in some ways productive association of nations, nobody thinks that it has the least military, economic, or even political significance. In the forties or much of the fifties, this was not realized, so the changes came rather later, I think. I'm not sure that's got much to do with being born in the thirties.

SMITH: Well, certainly one of the big changes that occurs in the fifties and sixties is the amalgamation of British and American popular culture, which is perhaps the completion of something that was underway before.

RENFREW: Yes. I haven't thought about this in detail, but if you ask when British and American popular culture became amalgamated, obviously Hollywood had a huge impact much earlier. There did remain a British film industry in the postwar years, but it then went into decline. In some ways the amalgamation I suppose could be associated with the arrival of rock music, because from then onwards British pop music was in a rock context. Certainly I have very clear memories of that, because when I first went to the United States, when I was still at school, Bill Haley's "Rock



Around the Clock" was top of the pops in the United States.

SMITH: That's 1955.

RENFREW: Right, there we are. So I came back myself greatly persuaded . . . in fact I still have the LP of Bill Haley and the Comets which I purchased in the U. S. in that year. Then a little later of course it swept Britain, and Elvis Presley was not long afterwards. From then onwards, certainly, there was strong convergence in the culture. It's true of course that the Beatles went the other way, but it was still part of the same constituency, really.

SMITH: Part of the convergence was that British actors and British musicians think of the United States as their natural market.

RENFREW: Yes, that's right. But when we say "pop culture" in that respect, we mean popular music, really, and then it goes on to clothing and so on. Whether that has a radical effect in other fields is open to discussion.

SMITH: But there are people who argue that Britain shifts its orientation in the fifties from the Continent towards the U. S., in terms of intellectual life.

RENFREW: When was Britain ever related to the Continent? During the war we were allies of the U. S., but that doesn't necessarily imply any great intellectual rapport with the Continent. It's open to question to me. I can't think of any postwar period when Britain was ever in any serious intellectual relationship with the Continent, so I don't see the force of that. In political terms the special relationship



may have had an existence earlier, but clearly it was forged by Churchill and Roosevelt, and then it had its ups and downs, but it remained as something to which lip service was paid. It may even have had some existence at the time of Macmillan and Kennedy; it was always a reality on the British side. Well, indeed, Thatcher and Reagan were said to have a good rapport. It's only really since the end of Reagan's terms that the notion of the special relationship has become totally meaningless.

SMITH: To put this in more ideological terms, there are certainly people on the Left in Britain who have felt dismayed at the increasing influence of the U.S. in post-World War II Britain, or have felt that somehow or other that was not a positive thing.

RENFREW: But American centrist politics have always appeared well to the right of the British counterpart, and those of the Left in Britain have always been deeply skeptical of Democrats as well as Republicans. They've had occasion to express their skepticism at those times when there has been a strong political relationship which would therefore excite the ire of the Left. Clearly, that would begin to be much more formalized when Winston Churchill made his Fulton, Missouri, speech and the Iron Curtain was recognized, that must have been the moment when the Left would have been very much at loggerheads with most United States political currents, whereas before that there may have been some notional, ideational brotherhood, as allies all united together under the leadership of Stalin as well as Churchill and Roosevelt



against the Reich.

SMITH: There is a curious phenomenon in the fifties and sixties of British and American culture becoming more synchronized with each other, and at the same time there is the rise of third force thinking, or New Left thinking, where Britain and Western Europe could play an independent role of both the Soviet Union and the United States. These were intellectual currents that were undoubtedly prevalent in the universities as well as in London.

RENFREW: I haven't thought deeply about these matters, but the notion of a third force outlook must surely correlate strongly with the realization that Britain was no longer a foremost power in the world. You wouldn't speak of a third force at the time when Britain led an empire which the United States could only envy, as it were. So you wouldn't begin to speak in such terms until you've had the realization that Britain was no longer a world power. I think in many ways a decisive moment was in 1956 with Suez, because so many things seemed to go wrong there; that was the first time I think that the United States really repudiated Britain. John Foster Dulles actually opposed the Franco-British alliance, and indeed collusion with the Israelis, against Egypt. So that was clearly a decisive moment. I know the special relationship was much spoken of after that time, but that was really the end of any true special relationship, wasn't it?

SMITH: Actually, I don't know that there ever was a special relationship, because we

1. The first part of the document discusses the importance of maintaining accurate records of all transactions and activities. It emphasizes the need for transparency and accountability in financial reporting.

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4. The fourth part of the document discusses the implications of the findings. It suggests that the results have significant implications for the field of study and may lead to further research in this area.

5. The fifth part of the document concludes the study. It summarizes the main findings and provides a final statement on the importance of the research.

do know that second to containing Russia, America's foreign policy goal after the Second World War was the dismantling of the British, French, and Dutch empires.

RENFREW: I may be wrong, but certainly during the Second World War Churchill had the impression that he was striking up a great rapport with Roosevelt, and I can well imagine that was so. So there clearly was a special relationship then. I don't actually know what contacts Attlee had with Truman.

SMITH: Certainly the movement towards NATO, which started with the postwar military alliance between Britain and the United States, represented a special relationship, but American policy was at that time already beginning to lay the ground work of a common market, which Britain was skeptical of. So there were cross purposes going on both in terms of the fate of the British empire and in terms of what should happen to Western Europe.

RENFREW: But one of the great unifying thoughts was the perceived Russian military threat. The Berlin airlift was really something significant, because that did require a great deal of effort; it wasn't just rhetoric, there was actually something that was being done. There was a real threat that if it wasn't done and wasn't maintained a lot of balances would change and Berlin might well fall.

SMITH: Yes, and the British and Americans had unilaterally imposed the monetary reform on their zones in occupied Germany, which was a factor leading to the airlift, so they had a very close policy, clearly, in terms of what they saw should happen in

1. The first part of the paper discusses the importance of the study and the objectives of the research. It highlights the need for a comprehensive understanding of the subject matter and the role of the researcher in this process.

2. The second part of the paper presents the methodology used in the study. It details the data collection methods, the sample size, and the statistical techniques employed to analyze the data.

3. The third part of the paper discusses the results of the study. It presents the findings of the research and compares them with the existing literature. The results show that there is a significant difference between the two groups.

4. The fourth part of the paper discusses the conclusions of the study. It summarizes the main findings and provides recommendations for future research. The study concludes that the results are promising and that further research is needed to confirm the findings.

5. The fifth part of the paper discusses the limitations of the study. It acknowledges the weaknesses of the research and provides suggestions for how to overcome them. The study acknowledges that the sample size was small and that the results may not be generalizable to other populations.

6. The sixth part of the paper discusses the implications of the study. It explains how the findings of the research can be applied in practice and how they can contribute to the field of study. The study suggests that the findings can be used to develop new interventions and to improve the quality of care.

7. The seventh part of the paper discusses the future research. It identifies the areas that need further investigation and provides suggestions for how to conduct this research. The study suggests that future research should focus on the long-term effects of the intervention and on the role of the patient in the process.

8. The eighth part of the paper discusses the conclusion of the study. It summarizes the main findings and provides a final statement on the importance of the research. The study concludes that the research has provided valuable insights into the subject matter and that it has contributed to the field of study.

Germany.

RENFREW: But how far that related to general intellectual movements . . . you were suggesting there was a link, and as an intellectual historian it would be your business to do so, but I become skeptical. I'm not being skeptical about your field, but there have been a number of archaeologists like, say, Bruce Trigger, who've thought to suggest that certain trends in archaeology were clearly related to fear of the atom bomb or broad issues like that, and really I found those to be totally gratuitous observations of no merit.

SMITH: Right, or looking at Binford in some way as an agent of U. S. post-World War II cold war consensus politics.

RENFREW: Yes, complete rubbish, just no case for that at all.

SMITH: There was certainly a profound change in the student bodies of British universities after World War II, though I don't think it was simply the Labour government's policy. I think it would have happened if Churchill had been reelected in '45.

RENFREW: Well, indeed, I think Mr. [Richard Austen] Butler's reforms laid many foundations for education in the postwar period, and that was a rather enlightened Conservative approach, and then, as you say, under Attlee further positive things happened, that's right.

SMITH: You entered into a system that was already developing. Did things seem



fine to you, or in crisis?

RENFREW: Absolutely not in crisis. I wasn't particularly aware of the politics of education, but as I mentioned to you, by the time I was eleven, in 1948, the eleven plus system was established whereby if you could pass the entrance exams you could go to a grammar school. This was therefore a selective system; it wasn't the comprehensive system which Labour later introduced, but it was a system that allowed many people to go to grammar schools, who weren't able to pay. St. Albans School was a sort of lesser public school; indeed, in some ways it wasn't really a public school in the sense of having a very intense social life or sporting life, mainly because the great majority of the pupils were day boys, whereas in a real public school about half the people are boarders. One wasn't particularly aware of class issues at St. Albans. Most of the pupils were people who commuted in, usually shorter distances than my own. So there was no strong class awareness, but it was a good education, and it didn't cost those who were attending anything at all.

Then, as we mentioned earlier, if you obtained the qualifications for university entry, you had a state scholarship which was means tested, which meant that your parents had to pay your upkeep if it was reckoned they could afford to, but those university students whose parents couldn't afford to pay received quite a good grant on which they could live. So, again, from the university student's point of view, life was fairly unproblematic. Of course, looked at in retrospect, that was because it was

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2. The second part of the paper presents the methodology used in the study. It details the research design, the data collection methods, and the analysis techniques employed to ensure the validity and reliability of the findings.

3. The third part of the paper discusses the results of the study. It presents the data collected and the analysis performed, highlighting the key findings and the implications of the research.

4. The fourth part of the paper discusses the conclusions drawn from the study. It summarizes the main findings and the implications of the research, and provides recommendations for future research.

5. The fifth part of the paper discusses the limitations of the study. It identifies the strengths and weaknesses of the research, and provides suggestions for how the study could be improved in the future.

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7. The seventh part of the paper discusses the ethical considerations of the study. It outlines the measures taken to ensure the ethical treatment of the participants and the integrity of the research.

8. The eighth part of the paper discusses the funding of the study. It provides information about the sources of funding and the role of the funding bodies in the research.

9. The ninth part of the paper discusses the dissemination of the research. It outlines the plans for sharing the findings of the study with the academic community and the public.

10. The tenth part of the paper discusses the future of the research. It provides a vision for the future of the field and the role of the researcher in this process.

quite a small proportion of people who went to university, so it could be afforded at state expense. The crisis that has come about in Britain over the past seven or eight years is that the university population has expanded so greatly that not only the Conservatives, but also Labour are now saying that we can't really continue to have a system whereby maintenance is paid automatically, and perhaps even where tuition is paid for all. As I'm sure you know, the government, finding the system about to collapse, instead of doing something about it took the easier expedient of setting up an inquiry, which is now beginning. In two years time, after probably a lot of damage has been done, they will no doubt make some rather obvious proposals.

SMITH: Yes. You have an impressive knowledge of contemporary art, and I'm wondering if that was a taste that developed as you were a young person in college, or after graduating, and how you went about familiarizing yourself with what was going on in contemporary culture.

RENFREW: Yes, I am interested in contemporary art, and I was already as a schoolboy, probably under my father's influence. As I mentioned, we'd go and look at art galleries. He wasn't particularly interested in contemporary art, but came to the view that it was natural to look at paintings. Indeed, I had one very good art master at school, Mr. Bob Tanner, who was very sympathetic to the circumstance that I didn't seem to have any great gifts for painting or drawing. During art classes, once or twice a week, I would mainly look at the books which he had there, and so without

trying very hard I got familiar with the works of Donatello or Ghiberti, as well as Michelangelo. When we went on that great visit to Italy in 1949, my first visit, one thing we did happen to do was look at many of the works of Michelangelo, sculptures as well as the Sistine Chapel, and so without ever reading about the matter very seriously I was familiar with these things.

I think also another sheer piece of good fortune was that while I was a schoolboy we had living in Welwyn Garden City a friend of my parents, E. J. Power, who was an industrialist. He owned what in modern terms would be a small radio and television manufacturing firm, Murphy Radio, which underwent great growth. Ted Power was a very remarkable man. He developed a very strong interest not only in contemporary art, but in really very up-to-date contemporary art. He had never been to university. He worked in the Merchant Navy in the first war, I think, and then retired from that with small funds and worked in Murphy Radio, bought part of the equity, and then ultimately became the principal owner. He had what in retrospect was an absolutely staggering collection. I remember when he was first buying École de Paris. Not Picasso and expensive things like that, though he did have a very beautiful Bonnard which I remember, but works by people like De Staël and Dubuffet. And he was collecting people who we today would call American abstract expressionists, so already in the early fifties I was looking at sculptures by Giacometti, and paintings by Clyfford Still, Mark Rothko, and Jackson Pollock, which this person



in a small house in Welwyn Garden City had on his walls. Then when he sold up from Murphy Radio he moved to London and had a larger flat. He was the first person I think ever to buy a painting from Barnett Newman.

Not many people in Welwyn Garden City were very interested in his collection. I remember his imploring my father and myself to borrow some of these paintings because if we just looked at them and had them on our walls we would see how good they were, and these were works by Rothko and Pollock . . . no, no, we didn't have space for them. But I got to know his collection well. There was a beautiful nude by Bonnard which he later sold because he was more interested in genuinely contemporary work, and by then Bonnard was more or less an old master.

(After finishing school and before going off to do National Service in 1956, I had a wonderful six months in Paris, staying with a French family and spending my time visiting the great museums and galleries, as well as the smaller commercial galleries on the Rive Gauche. Again, in 1958, I had another six months in Paris, and I knew the École de Paris fairly well by then.) So already, before I came up to university, I had seen a lot of really good, genuinely contemporary painting.

When I was here in Cambridge as a student I spent quite a lot of time writing art reviews for the student newspaper *Varsity*. At that time there was a very good gallery in Cambridge, run by the Arts Council, which had Arts Council traveling shows. I remember one first-class exhibition of works by Kurt Schwitters, which I

THE HISTORY OF THE UNITED STATES

OF THE UNITED STATES OF AMERICA

BY

JOHN F. JOHNSON

OF THE UNIVERSITY OF CHICAGO

AND

OF THE UNIVERSITY OF CALIFORNIA

AND

OF THE UNIVERSITY OF MICHIGAN

AND

OF THE UNIVERSITY OF TEXAS

AND

OF THE UNIVERSITY OF VIRGINIA

AND

OF THE UNIVERSITY OF WISCONSIN

AND

OF THE UNIVERSITY OF ILLINOIS

AND

OF THE UNIVERSITY OF MINNESOTA

AND

OF THE UNIVERSITY OF NEBRASKA

AND

OF THE UNIVERSITY OF KANSAS

wrote about. This was about 1958 or 1959, so he was already very well known, although not as well known as he is now. There were one or two private galleries exhibiting works by various artists, some of whom are well known now. The first writings I ever published anywhere were those reviews of art exhibitions. So I did become very enthused then, and that was when my mother provided the money for this painting points out, one of the earliest paintings I acquired, by William Turnbull. I think it was when I got my first degree, in 1961. Later on, living in Sheffield, rather remote from what was going on in London, and also having no money at all to buy anything, I didn't really collect at all, but since being back in Cambridge, and particularly being in this place (the Master's Lodge), where there's room to swing a cat, as it were, or hang a picture, I have collected a little more coherently.

SMITH: So these paintings are your personal collection, as opposed to the college's?

RENFREW: Yes. In the Master's Lodge there are a few portraits of old masters, in the personal sense, and there's also a very beautiful, two-hundred-year-old view of Athens in the dining room, which belong to the college, but all the contemporary works are ours.

SMITH: Do you follow what's happening in the arts relatively closely?

RENFREW: Yes, I do, but one of my weaknesses, though not a total weakness, is that I've never really read very extensively in the field of contemporary art. Certainly, although I've never made any profound study of the old masters, I really have seen

[The text on this page is extremely faint and illegible. It appears to be a list or index of items, possibly names of people or places, arranged in several columns.]

most of the galleries of Europe and looked at some of the work there. In the world of art, in some ways it's a weakness and in some ways it's a strength to judge through one's eyes, visually, rather than through reading. When I was in Paris in '56 and '58, as I was mentioning to you, I spent a lot of time going round the galleries, and for my twenty-first birthday I had a nice present from my father of a couple of hundred pounds, which in those days was quite a tidy sum, and I bought a number of lithographs. I still have a nice collection of lithographs, and a lot of posters which were original lithographs to advertise exhibitions. I have nice original posters by Matisse and Picasso and Braque, a couple of beautiful lithographs by Jacques Villon, and a very beautiful little lithograph by Dubuffet, for which I paid five pounds at the time. That was when I began buying one or two paintings, in 1958. I've tried to keep up since then, but I've never been at the forefront of the latest currents of thought, which has left me at a disadvantage with some people, for instance, Joseph Beuys. Are you an admirer of Joseph Beuys?

SMITH: Yes.

RENFREW: Well, I've never visually greatly warmed to Joseph Beuys. He was billed to give a lecture here in Cambridge about five years ago, and I made the terrible mistake of going, and I was so appalled by the self-importance of the man that it's been impossible for me ever to look at his work with a charitable eye. On the other hand, I very much admire Richard Long, who is a remarkable artist. He has done



very original work in new directions. But I haven't really had the time or the opportunity to keep operating at an international level. I think to know what's going on in a number of countries, to know what's actually happening now in the American art scene, you either have to read a lot or you do have to go across to New York and maybe to California now—no longer a province—twice a year or something to know what's going on.

SMITH: Well, usually things do come to London, to the Serpentine Gallery at the very least.

RENFREW: Yes, I see what happens there, certainly. One of the very pleasant features of living in Jesus College has been the opportunity of getting to know a great many artists personally, because we have a purchasing scheme, although a modest one, and it's very natural, and indeed quite a good thing, to get to know the artist. It facilitates the purchase in various ways. It turns out that many artists very much enjoy coming to a college, and they're pleased if their work is bought by a college. Quite simply, all artists like their work to be seen, and if somebody's going to buy your work and put it in their house, it's nice, but if it's being bought by an institution with 650 very bright people, some of whom will look at it, then it really has to be good news. So the college now has very strong relations with a lot of artists, some with very strong international reputations, like Richard Long or Barry Flanagan or John Hoyland. So that's been very good fun, and it's allowed one to develop that

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interest in a very positive way.

SMITH: Were you personally responsible for developing an art program at the college, for instance, commissioning the Paolozzi sculpture?

RENFREW: We have a committee, and so it's not something that one could do on one's own in a college; it's been a team enterprise. The curator of works of art is a fellow in engineering who is very interested. Of course, it is easier for the master to take an initiative if an artist is speaking in the college and say, "Well, let's invite everybody back for a drink in the Master's Lodge and ask the staff to lay on some wine." One has to pay for it, but it's not an expensive enterprise to offer twenty or thirty people a glass of wine. So it is easy to be in a position to have a positive effect; it was shortly after I came to the college that we developed a more ambitious program.

There was already a very good student purchasing scheme, which I think came about largely through the influence of a fellow in architecture, a leading architect after the war, Sir Leslie Martin, who designed the Royal Festival Hall, which was one the big buildings in postwar Britain, finished in 1951. He was a friend of Henry Moore and Ben Nicholson, and he was instrumental in building up a good student loan collection, and that continues, but we have far more works which belong to the college now, and that's rather a good thing.

SMITH: What about your interest in contemporary literature, poetry, film, and



music? Were you following those developments as well?

RENFREW: Like most people I enjoy going to see a film. When I was a schoolboy, in my late teens, and then as an undergraduate, I used to go to the theater a lot, and until we left the south in 1965 to go to Sheffield, I kept up very well with what was happening in British theater. It was less so since that time, regrettably, partly because Sheffield is not a very good jumping-off point to see what's happening in London, but, also, one of the sad things about academic life is that it does take up time, and it's difficult to go regularly to the theater or to the cinema if you're busy trying to write things. There's a real conflict there. But the theater I've always really enjoyed, and like most people, the cinema to a good extent. I've never read deeply in contemporary fiction or poetry, though I do like some of Britain's great twentieth-century poets. I'm not *au fait* with what new poets have been writing over the past five years.

I've always felt myself surprised that while I have no difficulty at all in responding to abstract art in a constructive way, and looking seriously and quite constructively at any visual art, I don't have the same feeling of access where music is concerned. If I look at Picasso or Braque from the early years of the century, they are old masters; there's nothing new about them—I mean, let's go and see something contemporary. I scarcely see them as moderns, except in the sense of the modern movement. Yet, when I hear music from that period, Stravinsky for me is pretty



modern stuff, and anything post that, like atonal music, I do not feel at home with.

So I regret that, and it's a kind of ignorance on my part, but it's one that has not been easy to overcome.

SMITH: Perhaps we should return to archaeology.

RENFREW: I'm sure there's much more to be said about the visual arts. I am really an enthusiast I think I can say, but maybe we can come back to that when questions arise.

SMITH: I think we can come back to that, yes. After Quaterness, your next excavation was Phylakopi, on Melos.

RENFREW: Right, yes.

SMITH: I wonder if you could describe a little bit how you came to decide to do that and what you were looking for in terms of that excavation?

RENFREW: This was part of my long interest in the prehistory of the Cyclades, and still, indeed, in problems of the early bronze age of the Cyclades, because the work at Saliagos had made genuine contributions to our understanding of the neolithic period, and then the work at Sitagroi had been relevant to the neolithic and the early bronze age of the north Aegean, which had no bearing at all on the Cyclades. What the study of Cycladic prehistory needed and still needs is some excavations on settlement sites of the early bronze age in the Cyclades. There weren't really many sites that would be productive in that area, and certainly none that held out a confident promise of a



culture sequence that would be illuminating. Professor Caskey had already been excavating on Kea, as I mentioned, to very good effect, and his site at Aghia Irini had very good stratigraphy in the lower levels that was important for the early bronze age, but apart from that there was nothing obvious except Phylakopi, where the British School had done very good excavations between 1896 and 1899, and those had been well published in 1904. There had been less impressive excavation in about 1911, which had carried the work a little further forward.

[Tape VI, Side One]

RENFREW: We were talking about the stratigraphy of the early bronze age at Phylakopi. Well, those have been quite abundant finds, and they've been well published. The stratigraphy hadn't been very fully documented, so it seemed an excellent opportunity to return to that site and try and make something out of that. Because it was a multiperiod site, it was logical to have a series of objectives, so another objective was to try and think about the urban growth at Phylakopi, and there we wanted to elucidate the chronology of the fortifications. Also, it seemed like a good idea to try and learn a little more about the very late period on the site, which required digging towards the top, where we might find the latest levels. The pottery in the late bronze age didn't really look as if it changed very much; it required a much more profound understanding than was available before the 1940s to detect the rather finer chronological differences which have now become very relevant if we're talking



about the last days of the Mycenaeans—this was one of the weaknesses in Aegean archaeological research in the early years of the present century.

My own main objective was to learn more about the early bronze age, so we chose areas where early bronze age pottery should be available once one dug down. Useful material was found, but not a very rich stratigraphy, so although we had something to say, I don't think we're overwhelmingly wiser. Our general understanding of the site is better. We were able to redate the fortifications more accurately, and to redate the first frescoes on the site. There had been some misunderstandings of the stratigraphy by the original excavators; they had structured the chronology incorrectly, so we could put that right. We were able to redate a small palace there. We found a fragment of a tablet with a Minoan Linear A inscription, which was very interesting. The inscriptions didn't say much, but the existence of such a tablet there was interesting.

Then, by good fortune, at the top of the site we found a building which turned out to be a sanctuary. To find a site like that was unexpected and really very exciting, so, in a way, the focus shifted. Although the early bronze age material was interesting and worth looking at, by far the most exciting thing was to find an undisturbed Mycenaean sanctuary and to have the opportunity of digging it quite carefully and in detail. That's what we did, and two volumes have been published so far. The volume on the sanctuary is quite a detailed treatment that allowed one to think about a lot of



problems as well as say what we found. Then the environmental volume has been published, talking about Melos in its broader context. We still have to publish, long after the excavation, the remaining finds, which are just about ready for publication now.

SMITH: Did the Phylakopi excavation change or alter your theoretical thinking at all? Did it push you in new directions along that line?

RENFREW: I'm not sure that excavation itself necessarily does alter one's theoretical thinking very much, but it did to this extent: having found the remains of what in the colloquial sense was clearly a sanctuary, one had to ask oneself much more clearly what a sanctuary is, and how, from the material remains alone, you can talk about the religious beliefs and practices of prehistoric communities; this has always been a problem area in archaeology, and one not really very well dealt with. So it was necessary to develop some theory to cope with those matters. When I published the sanctuary, I called it, perhaps a little boldly, "The Archaeology of Cult," and then the subtitle was "The Sanctuary at Phylakopi." The essays in that book that were most interesting to write were the early ones, on how one tackles the problem of interpreting what has been excavated, and then the later chapters, when I really had to sit down and achieve some sort of synthesis of Aegean late bronze age religion. It was a very useful learning experience, because I had always specialized in the neolithic and early bronze age, and there's really a great deal to learn about the late



bronze age, which I didn't know very much about. There's still a great deal to learn; there are many subfields. Studying Mycenaean pottery or Minoan pottery at a detailed level is a very specialized field, and there I am a complete beginner and amateur. Then of course if you are talking about the scripts, I'm not even a beginner or amateur; it's just somebody else's field, so to speak.

SMITH: But these are collective endeavors, in any event.

RENFREW: That's right, yes, but I did have to familiarize myself with Mycenaean archaeology, and particularly Mycenaean religious sites in order to be able to write a review of Mycenaean chronology, so it was a good learning experience. Also, apart from learning what was known, it was clear that thinking about bronze age religion in the Aegean was totally rooted in nineteenth-century thought. What Sir Arthur Evans wrote in 1900 or 1904 was to a large extent following grounds laid by Tsountas in 1890, which followed precursors like [James George] Frazer, or even [Johann Jakob] Bachofen; you know, ideas which you wouldn't take seriously today, but they had been mediated through decades of Aegean archaeological scholarship and were still the accepted wisdom. So that was a very interesting task, to look at these ideas, question them, and try and formulate some new thoughts.

SMITH: Well, if we take that as a case example, here you have a bunch of stuff that you've dug out of the ground, and then you have a sort of natural skepticism, or maybe it's a trained skepticism, to read the preceding texts critically, and you have a

THE HISTORY OF THE
CITY OF BOSTON
FROM THE FIRST SETTLEMENT
TO THE PRESENT TIME
BY
JOHN HUTCHINGS
OF THE BARRISTER AT LAW
IN THE SUPREME COURT OF JUDICATURE
IN GREAT BRITAIN
AND OF THE COMMONS OF GREAT BRITAIN
IN PARLIAMENT ASSEMBLED
IN THE YEAR 1764
LONDON: Printed by J. DODD, in Pall-mall.
MDCCLXIV.

theoretical commitment to try and understand religion as one of several social processes that are taking place. How do you integrate these objects and your reading of the objects with your critical reading of previous archaeological literature, and your still-forming theoretical concerns, to yield some conclusions?

RENFREW: I have never been theory-led in the sense of starting off with a very well-developed theoretical framework. I doubt if that would work very well as an approach anyway. To some extent, the questions one is asking and the approach one is developing will be partly prompted by the material itself, which seems to me quite proper, and, in a way, it's fair to say that this is the way a physicist or a chemist would operate. To use the word "scientific" is really counterproductive; it suggests that one is conforming to some well-established principles of investigation, but I think the reverse is true in science also; it has always been the case that the philosophy of science has lagged far behind the practice, and although philosophers of science may argue otherwise now, scientists have always had to make judgments about what to do next on the basis of the situation in which they find themselves. In that sense one has data one is seeking to make sense of and place in a wider context.

You are right that the excavation was conducted first, so, really, the hypotheses were few. The intention was to recover data in a precise way. There were many smashed vessels and figurine fragments that had to be mended up so that they fitted together and one could record the stratigraphic links between them. In the

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field it was quite an intellectual achievement simply to get a fairly coherent and confident assertion of what the overall sequence of this building was so as to produce a coherent local history for the building. Then there was the question of what it all meant, and that did involve reading what had been written and looking at the similarities and differences between our material and material elsewhere, and then asking how one proceeds.

Again, trying to adopt a skeptical position, the first question I asked was, "At what point would it be warranted to conclude that we're dealing with material relating to religion?" I think most Aegean archaeologists would have said, "Well, of course we know that this symbol relates to that symbol, so it's the well-known Minoan horns of consecration," or whatever, but because I was skeptical about the well-known Minoan double axe, and what it meant, or the tree of life, or the pillar, or whatever it may be, I preferred not to accept any of these things on trust. The interesting thing was, if you looked at the site in itself and looked at these human representations, which by convention you call figurines, and the various finds, it was clear we had a concentration of symbolic materials of a kind not much found on the rest of the site, but was it really religion? I remember giving a lecture here in Cambridge discussing this issue, and a very distinguished Aegean scholar, John Chadwick, not I think normally that skeptical, but perhaps accepting the skeptical issues which I had introduced, said, "How do you know it was more than a Mycenaean toy shop?" And

1. The first part of the paper discusses the importance of maintaining accurate records of all transactions. It emphasizes that proper record-keeping is essential for the transparency and accountability of the organization. The text outlines the various methods used to collect and analyze data, ensuring that the information is reliable and valid.

2. The second part of the paper focuses on the implementation of the proposed system. It details the steps involved in the development and testing of the software, as well as the challenges encountered during the process. The authors highlight the need for a thorough understanding of the user requirements and the importance of iterative development.

3. The third part of the paper presents the results of the study. It compares the performance of the proposed system with existing solutions, demonstrating its superior efficiency and accuracy. The authors also discuss the limitations of the study and suggest areas for future research.

4. The final part of the paper concludes with a summary of the findings and a discussion of the implications for practice. The authors stress the importance of continuous improvement and the need for ongoing evaluation of the system's performance.

that does underline a very serious problem which emerges when you are talking about manifestations of religion and cult, and that is the distinction between play and symbolic systems taken seriously, mainly religion.

If you think of many of the major installations for play, such as they are, they often don't differ very much in their formal properties from religious installations, so that if you were excavating, it would be very difficult to judge which was which. If you excavated a major football stadium, clearly it was there for many people to take part in something, but what they were taking part in would be difficult to judge. Indeed, ultimately, if you ask yourself in behavioral terms, as an external observer, what is the difference between a major football match and a religious ritual in a cathedral, and how you know which is which, it is quite difficult to decide exactly what are the criteria. The component which as an external observer or as an archaeologist you don't know, which is the component of faith and belief, is what the archaeological record doesn't give you great access to.

As for Phylakopi, judged on its own, if you were not aware of finds elsewhere in the Aegean, you might well conclude that what we were excavating was a sanctuary, but you really wouldn't know. It isn't until you are able to compare that site with other sites in the Aegean, particularly in Crete, where you have a richer iconography, that I believe you can find arguments that would lead you to conclude, "Yes, this is actually a religious site." It leads on to a whole number of interesting



questions, which in general have not been addressed by archaeologists, because the earlier generation of New Archaeologists didn't bother much with ideational things and weren't troubling themselves with religious centers, whereas the traditionalists would say, "Well of course it's a shrine; Sir Arthur Evans told us that a hundred years ago, there's no need to question that." So they would completely dismiss the theoretical or methodological heart searchings that one might today wish to introduce.

There are real possibilities of new ground to cover there. It was an interesting exercise, and it deserves to be carried further, because the distinction between ritual and play can be quite difficult to distinguish. Play can involve ritual, of perhaps a nonserious nature, and the definitions begin to be very difficult. Then of course you have scholars like Johan Huizinga, the author of the book *Homo Ludens*, who are arguing that most human activities may be regarded as play.

SMITH: Or Spencer, again. Spencer's theory of religion was that it was the organization of playful activities towards—

RENFREW: Did Spencer say that? I've missed that entirely, so superficial has been my reading. I might have found that very helpful and useful. Where does Spencer say that?

SMITH: In *The Data of Ethics*.

RENFREW: Oh, really? Well, I have to look at that. I might have found that a very

1. The first part of the document discusses the importance of maintaining accurate records of all transactions and activities. It emphasizes the need for transparency and accountability in financial reporting.

2. The second part of the document outlines the various methods and techniques used to collect and analyze data. It includes a detailed description of the experimental procedures and the statistical analysis performed.

3. The third part of the document presents the results of the study. It includes a series of tables and graphs that illustrate the findings of the research. The data shows a clear trend of increasing activity over time.

4. The fourth part of the document discusses the implications of the findings. It suggests that the results have significant implications for the field of study and may lead to further research in this area.

5. The fifth part of the document concludes the study. It summarizes the key findings and provides a final statement on the importance of the research.

helpful insight had I been aware of it.

SMITH: But I'm being playful in mentioning that.

RENFREW: Not necessarily so. There are two stages of observation. If you were an anthropologist observing a society, but you did not understand the language, so you could not make use of oral information in any form, nor elicit it, what would you conclude about that society? Well, no anthropologist would be in that position, because they all claim to have access through verbal interaction. But then if you go one more remove and try to observe a society where you can't even see the people operating, and you have to look at the material remains, what can you infer from that? Well, those are the essential archaeological problems, so you have to face them, even though no anthropologist would pause for a moment to answer such nonsensical questions. If you have no direct insight into people's intentionality, no orally communicated insight, nor visually communicated insight in terms of action, and you can only look at the material remains, really serious questions arise which are very interesting.

One question that comes into my mind, which is always regarded as obvious, is how do you decide whether something you are looking at is a depiction or not? You don't know if it's humanly produced, you perhaps assume that it is, but at what point do you recognize a line on a surface as being a depiction? I haven't researched the issue profoundly, but it's clearly a very difficult question. If you recognize subject



matter, you recognize a depiction, but that is an entirely subjective view. This problem came to my attention through a very strange lady who came to see me. She had worked in Germany in rock gorges, and she was interested in megalithic monuments. She would show me a photograph and point to configurations of what she thought was clearly a human face, or a woman facing a man and so on, and I thought, "I can't see this at all. Is this woman crazy?" Then she'd show me an overlay where she had traced these representations on the rock. Of course when you saw the tracing you saw the face and figures, and then she'd remove the tracing and then you would say "Ah, yes, that's where the face is . . ." Ultimately, it was clearly nonsense, but it did raise the question of how one decides whether something is a depiction or not.

SMITH: But in your handbook you give the example of the early Venuses, which you can assume to be Venuses because there are clearly human chisel marks on them. You can read them as female figures, or you can just read them as rocks, but the only thing that allows you to make a determination that they must be some form of representation is the fact that somebody's gone to the trouble of altering—

RENFREW: Well, I think that is an important part of the argumentation, but if you're talking about a rock surface, you can have chisel marks on rock that are not part of a depiction. Indeed, you might have, although it's less likely, three-dimensional objects which have been altered by human agency and you could document that, but that



doesn't necessarily make them depictions. There is a man, who mercifully has stopped writing to me, but for about the past year he has been writing to me once a fortnight, being vehemently abusive about the archaeological establishment. He will not accept that the paleolithic flints which he has are actually depictions. I threw it all away, but I now realize in this very context it's of methodological interest: here you have these flints, many of which are no doubt chipped stone objects, but this has got a human face on it, hasn't it? There you see, there's no doubt that they are altered by human activity, but the question is, are they depictions or not?

SMITH: What about an artist who may pick up a piece of wood or a piece of rock that has a stain on it that they then interpret as a depiction? They say, "This is a Madonna." And once they've said it's a Madonna—this is a Duchampian dilemma—you have to see the Madonna. You oscillate in and out—

RENFREW: You have to see it, and of course, the moment they declare it to be a work of art you have to accept that. There is no better definition of art than simple declaration, just saying, "That's a work of art." But nonetheless, you and I, if we exercise our common sense, both know that that was found and is a product of nature. We both know that it isn't a depiction of a Madonna. We may well know that the artist asserts it to be such, and so by various conventions we may well feel obliged to accept it as such, but you know and I know that it isn't.

SMITH: No, but the difference is, if an artist says it's a depiction, even if it's made by

[Faint, illegible text, likely bleed-through from the reverse side of the page]

nature, we accept it as a depiction, whereas—

RENFREW: Who does?

SMITH: Society in general, the art world.

RENFREW: I don't think that's true. Mr. [Joseph] Beuys puts something in a box, and we all say, "How wonderful, Mr. Beuys. You've put that in a box. It's a great work of art." But if Mr. Beuys picks up a flint and says, "Look, this is the Queen of Sheba," we don't all say, "Mr. Beuys, how wonderful! That's the Queen of Sheba."

SMITH: But if Frau Jedermann comes along and says, "This is the Queen of Sheba," we'll say, "Oh sure," and wish that she'd go away or something. So in that case it becomes a depiction because of the social institution that stands behind the assertion.

RENFREW: I don't see that. We may well accept it's an art work worthy of our respect, and as such we will accept it through a convention. "Yes, Mr. Beuys, that's a depiction of a Madonna," but you know and I know that he found it on the beach and it isn't a depiction of the Madonna, it's Mr. Beuys asserting that, in a manner which we respect.

SMITH: Okay, all right.

RENFREW: I mean, you can dispute that if you wish, but after all, when we say something is a depiction, we imply that the creative process, the process that produced that form carried with it the intentionality that it results in a depiction.

SMITH: But Mr. Beuys's point is that the creative process is in the imagination and



not in his hands.

RENFREW: We can agree on that, and we can see in it a fine work of imaginative creation, but if we retain a grain of skepticism, which I do, we know that in its process of formation it wasn't intended to be a Madonna. We can accept it to be a Madonna, but it wasn't made as a Madonna. If you believe in the magic of Mr. Beuys you can say that because he exercises magic, it's now a Madonna; you could have that view. You know *my* view of Mr. Beuys, so I'll just say, "Mr. Beuys says it's a Madonna," and out of tact I won't contradict the old gentleman.

SMITH: Okay. Perhaps we should end here. [laughter]



SESSION THREE: 17 MAY, 1996

[Tape VII, Side One]

SMITH: You mentioned that you'd like to say a little bit more about American influence on Britain.

RENFREW: Yes. It may be a little apart from personal reflections, but we were discussing the extent of American influence, and it dawned on me that it's really rather an interesting issue in a general sense to know when American influence on Britain began and in what ways. British influence obviously was strong on America in colonial days, but I'm not sure when British cultural influence on America declined, and if and when American cultural influence began to be exerted on Britain. I thought you yourself, since you are very clear on the concept of provincialism, would know. When *did* the United States cease to be provincial vis-à-vis the United Kingdom?

SMITH: Probably never, actually. [laughter]

RENFREW: I thought you took rather a cautious view on it; you seemed to think provinces survive provincially for quite a long time. We were talking [off-tape] about California, and you felt that California retained its provincial status longer than is often thought, beyond the time when it might have been claimed to have an autonomous effectiveness. But if we're talking about things coming the other way, clearly Hollywood, though it may in a sense be superficial, wasn't really superficial at all because all the population of Britain and elsewhere were watching American films.



When the British film industry declined catastrophically, then most of the films being looked at were American. Of course, television has built up, and I'm not sure that the opinion-making programs in this country originate from America. Certainly, if one goes to America one is appalled at the standard of American television, it really is dreadful. British television may have declined, but there's no doubt that it's much more interesting to look at, and I'm sure much more constructively opinion forming than I would say of American television. Of course we do have soaps, things like *Dallas* and so on, but in Britain at the moment it's the Australian soaps that are more influential than the American ones. So if we think of the British viewing public as watching television more than the cinema, then I'm not sure that the American cultural influence is so massively strong. Economically that may be so, politically, undoubtedly it is so, but if we think of the direct influences the United States has had on Britain, clearly there were the G.I.s in Britain during the war, which was real, but it was not a massive influence, so it does begin to come down to things like pop culture, pop songs and so on.

It occurred to me that there the influence is almost entirely a transmuted black American influence. If we're talking about the effect of music, first of all jazz, and then rock music, although it may have come to us partly through Bill Haley, who was certainly white, nonetheless, a lot of the dynamism has come from black Americans. Certainly, if you look at the influence of American pop culture, most of the speech



innovations coming into the English language from the United States are indeed ultimately of black origin, aren't they? And the great success of rap, which is having a massive influence on vernacular speech in Britain, is essentially of black origin, though it may often be transmitted, as rock largely was, through white performers. So that's rather a special area. The same is true of dress: if you look at the influence of America on popular dress in the youth culture, again, it's largely black America.

We were talking about American abstract expressionism, and that I think was the first impact of the visual arts from America. Nobody really is much moved by Grandma Moses in this country, or even by Georgia O'Keeffe so it wasn't until the the American abstract expressionists came along that there was a massive input in that way. Maybe we'd have to have more serious talk about mass production of Ford motor cars or something; there I'm sure there are influences.

SMITH: An area that has been discussed considerably is the influence of American models on the development of British higher education in the post World War II period. Of course, Oxford and Cambridge retained their historical autonomy and traditions, but the red brick universities tend to follow a curious amalgam of an American and British model; for instance, the introduction of the Ph.D. as a major degree was not typical prior to World War II.

RENFREW: That may be so. It had never occurred to me, although I don't read particularly widely in educational theory; it's not a field I find arresting. There were



many civic universities like Sheffield, where I was, which was founded I think in the 1870s, and there has been a rather natural growth from there. With regard to the Ph.D., you may be right, but quite a few of those who aspired to be academics took their doctorates. Archaeology is a rather recently developed field, but Grahame Clark took his doctorate just before the Second World War. Glyn Daniel had certainly begun his doctorate, though it may have been interrupted by the war. So it was already an established path, although it's true that the notion that everybody who is going to go into teaching or into some higher realm needs a Ph.D. is something which has developed in England since the war, and I'm sure it developed earlier in the United States.

But the whole notion of a Ph.D. program is an autonomous one, and indeed it is still very different in most British universities. Certainly in Oxford and Cambridge you study with a supervisor, you write a dissertation, and that's it, whereas in the United States, so far as I am aware, you nearly always have a whole series of written examinations which you need to undertake before you proceed to the written dissertation. The systems are still quite different in that way, though it's true that now we are moving to having some structured teaching, and that may be American influence to some extent. The master's degree is something that has grown up rather recently in Britain.

So I think your question is an interesting one; I'm not sure how relevant it is to



the field of archaeology, or to my own experiences, but it would be an interesting debate to wonder what the cultural influences of America have been. Clearly, there was a point during the Second World War when physics research, the development of the atom bomb, did take place in the United States, and European expertise, including British expertise, was pooled. Then, partly for economic reasons, partly for cultural reasons, and I think it's been alleged also partly for deliberate reasons of scientific dominance, America took the lead, and it has been alleged that European powers, mainly Britain, which contributed to the atom bomb project, were somewhat excluded from later developments. I don't really know whether that's true or not, but it is partly an economic question of course.

SMITH: The New Archaeology has typically been described as an Anglo-American venture, particular to the English-speaking world, though with distinctions between the British and the American schools. I think you've been describing the New Archaeology in Britain as a distinct development.

RENFREW: A parallel development I would say, yes. We were talking a little earlier about philosophy of science, for instance, and I think there was a real awareness in Britain, or in parts of Britain, of methodological issues, and a wish to use some scientific modes of thought, which are particularly well exemplified by David Clarke. His very interesting book, *Analytical Archaeology*, was published in the same year as Binford's *New Perspectives* volume. David Clarke's statement is a much more



coherent and programmatic statement, and he is introducing systems thinking there much more comprehensively than Binford was doing. David Clarke may have been inspired by some of Binford's writings, but I think they were parallel movements, and you can see the ways that they differed. In Britain, for instance, there was much more emphasis on mathematical methods. Roy Hodson was one of the pioneers of mathematical applications in archaeology. On the other hand, Binford did have a much clearer idea of what was wrong or deficient about the traditional archaeology: too much focusing on specific artifacts or specific cultures. So Binford certainly gave the clearest expression of that idea. If we're talking about that, I think it's fair to say that the Scandinavians also had a role, the Danes and the Swedes, notably Carl-Axel Moberg, and to some extent the Dutch. The Dutch have always been very good in environmental archaeology, and much environmental archaeology was initiated in Scandinavia, such as the application of pollen analysis. As we were saying earlier, the environmental approach to archaeology was one of the components of the general progress of archaeology of which the New Archaeology was part.

SMITH: Did you participate in the international radiocarbon conferences?

RENFREW: Not extensively, no. I don't quite know why, because I was always very interested and active in the radiocarbon field, and I think I was one of the first to apply some of the dates in a systematic way to the logic of what was happening in Europe. But I didn't rush off to those conferences, I think mainly because they were



specialized conferences for those persons working in the laboratories, who were actually producing the radiocarbon dates, and the people producing the radiocarbon dates have very rarely been those who understood the archaeological implications. They were pretty well without exception physicists, or physical chemists, and only later statisticians, so that it was very much their wish to produce better and better dates. I knew a lot of them personally, and still do, like Rainer Berger at UCLA. Hans Suess was the American physicist who saw most clearly that tree-ring sequences gave a way of absolutely checking the radiocarbon calendar, because if you analyzed wood from those rings, it would give you a very close measure of how far away the radiocarbon date was from the calendar date, assuming that the tree-ring date gave you a reliable calendar date, and that hypothesis did turn out to be justified.

The work of Suess, and those people who were trying to match his work, gave a very clear expression of the best results of the radiocarbon method, so it wasn't really necessary for archaeologists to get personally involved in the details of how you get replicable counts, how you get better shielding to avoid distortions, how you look into isotopic fractionation, and a lot of the technical questions that one could be concerned with. At the time that the calibration of radiocarbon was coming to the fore, if one had Suess's latest information, one was really quite well informed.

SMITH: How closely did you participate in the dating of archaeological finds?

RENFREW: Well, I became involved in that when I began to see that there were



many things that were systematically wrong, as we were saying. I suppose the first radiocarbon samples that I organized were indeed those from Saliagos, but the objective there was to find out the date of Saliagos, and there was no way that the dates we achieved for that material were going to tell us anything very much about the radiocarbon dating method or its implications; that was just dating the neolithic of the Cyclades. But when it came to Sitagroi, the objective there was to obtain a really good radiocarbon sequence, see that it made sense, have it properly calibrated so that it could then be fed into the other Balkan dates to refute absolutely the suggestion which Milošević had made, for instance, that maybe there was something wrong with the radiocarbon dates from that area, some local factor that was distorting them. Well, that could indeed be seen to be wrong, so those samples were very relevant. Then later, I got samples from Malta and from Orkney.

All these samples were collected by me carefully, but then I had to negotiate with the relevant laboratories. There is a radiocarbon laboratory here in Cambridge that was able to accept some samples. There's one at the British Museum which I was using through their kindness, and also the Berlin laboratory took quite a few samples. At that time, and indeed still, there is merit in dividing samples and sending them to different laboratories. Not because one is doubting the laboratories, but just because it gives you independent checks and gives you some statistical ways of working with the data. So I did indeed arrange for some important samples to be

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processed, and this did prove very interesting.

A statistician colleague, R. M. Clark, and I became very interested in the Egyptian dates, because they were historically established, and then the radiocarbon dates had some discrepancies. So we worked together on a statistical analysis of those dates and we developed, mainly with his input but with some from myself, notions of "wiggle matching" in the radiocarbon calibration curves. This was fifteen years ago now, but at that time the question of the validity of radiocarbon dating was sufficiently crucial that it was a matter of worry. Now I think radiocarbon dating is more routinized and though the questions of precision still remain, they no longer really bear very much on what we're going to believe overall about prehistoric chronology. I think those questions are largely settled.

SMITH: And you also have newer techniques.

RENFREW: Yes, you have the accelerator mass spectroscopy, the AMS, dating method, which overcomes some of the difficulties, but it still has quite a large error margin, so it doesn't transform the picture altogether.

SMITH: I thought we might move into a discussion of your demic-diffusion model.

RENFREW: All right, yes. The demic-diffusion model really originated, as we were saying earlier, with Albert Ammermann and Luca Cavalli-Sforza, but I have used it, that is quite true.

SMITH: Right, and you have made some propositions based on it—



RENFREW: Yes, which have not been universally accepted.

SMITH: I think it was Marija Gimbutas who pointed out the irony that you, the great antidiffusionist, should embrace a diffusionist model.

RENFREW: Yes, she seemed to think this was very bad form, and it would be better if I would desist. That seemed to be her view of the matter, yes.

SMITH: But you noted that you got involved with this because the study of European prehistory was impeded by "hidden assumptions and submerged preconceptions" about the origins of the Indo-European languages.

RENFREW: It was much more than just that. The "impedence," so to speak, was not simply about linguistic matters. The whole understanding of the bronze age and the iron age of Europe has been totally impeded because the assumption was made, and is still widely made, that there were massive changes in Europe perhaps at some time in the bronze age, or maybe into the iron age. For instance, if we are talking about Celtic societies, or the northwest European iron age, which is colloquially known as the Celtic iron age, it was and is almost universally assumed that the Celts came from somewhere. Now, to dispute that assertion seems to many absolutely incomprehensible, but I think it is to be disputed. Clearly, if you are assuming that the Celts came from somewhere, you are assuming that many of the features of bronze age and iron age society were introduced into Europe from somewhere, often rather vaguely unspecified. This begins to get back to the Indo-Europeanist views, and



some of those who have written about social structures and belief structures, of the so-called Indo-Europeans. If you accept the notion that the Celts came from somewhere, naturally Celtic beliefs and social structures would in large measure have been imported from somewhere. If one allows oneself to be skeptical about that, as I am, then you can see how strange it appears, this whole notion that many of the characteristics of European society—European myths, European belief systems, religious systems, social structures, maybe even forms of poetry, quite apart from the actual structure of the language—are in some way external to northwest Europe, where we find the Celts when the Romans speak of them. So it remains a massive assumption.

When we were talking earlier about the diffusionist hypothesis, we were really talking about the neolithic and perhaps the early bronze age; for instance, the megalithic phenomenon. It is now widely accepted that the megaliths are a European phenomenon which occur earliest in northwestern Europe. It's still a cause for mild surprise—"Fancy that, they're older than the pyramids!"—but it is broadly accepted, certainly among archaeologists. But the impact of the thought that there may be no truth at all in the suggestion that the particularities of European bronze age and iron age society owe nothing to supposed immigrations of people and of ideas in the bronze age and iron age has not I think been grasped except by a few archaeologists who have sat down to think it through.



When I went to Oxford to give a paper on our finds at Phylakopi, I talked about the religious continuities from earlier periods: from the Greek neolithic into the Greek early bronze age, from the early bronze into the middle and late bronze age, so on to the Mycenaean period. I was talking about the changes in cult practices and cult symbolisms as a series of transformations starting in the neolithic, and that's really the only way you can, in my view, understand what was going on. Because there really hasn't ever been much systematic study about the history or archaeology of religion, most people, when they are talking about religions, imagine the complex of ideas of any religion as something that comes in ready-formed, as it were. It's very rarely seen that it must in some senses be a transformation of earlier ideas with new ideas innovating. Of course if you are talking about the Christian religion, the whole discussion is undermined by having to worry about whether the people one is discussing it with are Christians or not, which makes the whole discussion much more complicated.

Anyway, I was talking about this view of transformations in Oxford, and Professor Christopher Hawkes was there, one of the great traditional archaeologists, a great specialist in the bronze age and a very endearing man. Although he was by then old, he had a tremendous commitment and intensity about these issues. So I gave my paper, and he said vigorously, "This cannot possibly be right"—he didn't quite put it like that, but he meant that—"because everybody knows that the Greek



deity Zeus and the other Greek deities are essentially part of an Indo-European pantheon. Zeus is the same as Jupiter, *Zeus pater*, and you can find comparisons in Sanskrit." He was asking that since the religion of the Greeks was an Indo-European religion, how could it make sense at all to speak of it as a continuity from the earliest prehistory, that is, the neolithic prehistory? Of course, he would have said that with much greater force if I had been talking about the Celts in northwestern Europe, but the same observation was there. He was saying we *know* that these are Indo-Europeans, and by implication we *know* that the Indo-Europeans came in maybe some time in the bronze age, so how can anybody possibly argue for a continuity from the neolithic, and what nonsense is this about a series of transformations? They came in, and that was the transformation; it had nothing to do with these autochthonous and endogenous processual issues.

So, you were saying that demic-diffusion is diffusionist, which is certainly the case, but the point is, it's *one* episode of early diffusion. Once you've got the Indo-Europeans to Europe, then you are free to have them behaving in a processual manner, as it were, and then of course the demic-diffusion hypothesis itself is a very nice processual explanation; it is true it is an explanation that involves movement of people and movement of ideas, so in that specific sense it can be described as diffusionist, but it is unlike most explanations in terms of diffusion in that it is a very explicit model indeed, and it's a model whose dynamic is totally understood, so it is



entirely a processual model and in that specific sense it is not a diffusionist model.

SMITH: It seems to me that the power of the argument of the model is the way you bring together the evidence of three independent disciplines, all pointing toward the same conclusion.

RENFREW: I think that's at a second stage; the initial power of the model as a model is just that it's such a good model in archaeological terms. I imagine the other disciplines you are referring to in addition to archaeology are linguistics, and genetics.

SMITH: Right, yes.

RENFREW: First of all, it is a good model in archaeological terms because it really does explain. Now, that doesn't mean that it has to be right, and as you may well be aware, many archaeologists have said this isn't a very good explanation for the transition to a farming economy in Europe. They've said so mainly on the anti-diffusionist grounds which Marija Gimbutas was politely reminding me I ought to be continuing to advocate. There have been those who have said that the process of transition to farming in Europe was largely an autonomous and autochthonous one. People have tried to argue that the wheat, the barley, the sheep, and the goat might have been available in Europe; indeed, Grahame Clark pointed out that there were mesolithic cases of sheep in south France, but it does seem to be the case that the species of sheep used by the farmers of south France was an introduced species. So however antimigrationist, antidiffusionist one is, one has to admit that the basic



domesticates for the economy, the wheat and the barley, the sheep and goat, were indeed imported. But that's as far as you have to go; you can say that not one person had changed their place of residence. They had to be in contact through trade, so farming had to be acquired, the domesticates had to be acquired, and so too the techniques of farming had to diffuse, and this would be a true case of diffusion rather than migration. The wheat, barley, sheep, and goat would migrate by such a process, but the paleolithic humans, or mesolithic humans would be there, learning these things, obtaining these things through trade, and doing their own thing. The extreme indigenous advocates say that is what happened, and there must be some truth in that; it wasn't just a great slow wave of people moving across the continent as the model of demic-diffusion would suggest.

The truth is, the model of demic-diffusion doesn't require any preexisting population at all; it actually ignores the preexisting population. So the final answer has to be some mix of the preexisting population adopting farming and an incoming gradually spreading farming population. The argument really is, what degree of mix is it? Ninety-nine percent to one percent, or one percent to ninety-nine percent, or fifty-fifty ?

SMITH: Well, in Cavalli-Sforza and Ammermann's model there's an assumption, which I gather is a purely notional assumption, that there's a fifty-to-one ratio in terms of agricultural populations to hunter-gatherer.



RENFREW: It can be rephrased that the population of farmers, when they have achieved a natural level of population density, is about fifty times greater than the population density of hunter-gatherers in the same terrain. It doesn't follow that it's simultaneously so, but if you achieve a transition to farming, some centuries later, when it's regarded to be in some senses completed, the population density might be fifty times greater. That is their assumption, that's right.

SMITH: And this has been challenged by a number of archaeologists. One critic wrote that in fact neolithic farming was less capable of providing subsistence than hunting and gathering, and you see evidence of malnutrition in agricultural communities.

RENFREW: I think a lot of these detailed arguments have their validity. It is of course the case that there are some localized areas where hunter-gathering and the use of marine resources—where the fishing is particularly satisfactory, or there are very special resources—does allow greater population densities. Nonetheless, the population densities in Scotland in mesolithic times, for example, were extremely low from what we know. The population densities in early farming times in Scotland were probably still fairly low, but they were, on most people's estimates, significantly greater, probably by a factor of ten, than the mesolithic population densities had been. Most population densities are significantly higher for farming, once it's well established, than for hunter-gathering. If you take any terrain of a few hundred



square kilometers, that would generally be so.

SMITH: Perhaps we can look at the different types of criticisms. I think you've already dealt with the question of preconceived assumptions, but then you have another set of criticisms, what I would call the ideological criticisms, which could perhaps come from Gimbutas, claiming that your model has to be wrong because it doesn't explain the patriarchal and warrior nature—

RENFREW: That's right. She was a great leader of the sort of *Chalice and the Blade* approach—the profound difference of the society with a female nature. Marija Gimbutas had a special term for the cultures of Old Europe, meaning man and woman together in harmony. I forget just what she called it, but then she contrasted that with the patriarchal bronze age societies, stampeding in on their proud steeds from the Ukraine: basically ruffianly, ill-behaved, and the source of many of our ills. A lot of this is a world of myth and golden ages, like King Arthur, though it has nothing to do with King Arthur, but it's a world where a special light shines, as it were—the notion that you have one age when humankind lived in harmony and then that was superseded by another age when harmony was lost. I am a bit skeptical about this. Of course Marija Gimbutas achieved a considerable cult following because of these views.

I went to a conference in Malta on megalithic cultures and the temples and so on, and I think Gimbutas was there. Certainly she was a very strong presence if she



wasn't there in person, and there was a group of women archaeologists who were very caught up with all of this. To start with, they were very angry with me for being skeptical, but we talked about it, and we really began to get on quite cheerfully. I remember a number of them went down one evening into the hypogeum of Hal-Saffieni, and they held hands and absorbed the good vibrations coming from this place. Of course no men were invited to be present, though I'm not sure I would have dared to be present anyway. It irritates me that I can't remember what word Marija Gimbutas used, but it was a word meaning the happy marriage of the feminine and the masculine principles.

SMITH: Androgynous?

RENFREW: It's a made-up word rather like that, involving a root for male and a root for female, but androgynous obviously has other meanings in English and wouldn't have been suitable, so she quite reasonably invented a suitable word. (On recollection the word is "gylandry," using the Greek roots *gy* and *andr* for woman and man, but "gynandry" might have been more accurate.) We ended up on cheerful good terms, these profoundly sensitive ladies and I, but I do remain skeptical of this strong wish to read one's present concerns and indeed present aspirations so strongly into a particular period in the past.

SMITH: But since prehistory has been a screen for myth, probably going back as far as we would want to go back, but certainly for the last two hundred years, doesn't



that raise the question of how one then deals with the possibilities of mythic preconceptions lying within one's own model?

RENFREW: Right, well that's much discussed now. Indeed, even before the postmodern tendency arose in archaeology, which of course has given very coherent expression to these views, Jacquetta Hawkes I think it was, or it may have been Richard Atkinson, wrote the splendid article "God in the Machine"—they both shared the same view on the matter. This was the time when Gerald Hawkins wrote a series of articles about Stonehenge, the early sixties I suppose, explaining it in terms of a computer. There are fifty-six Aubrey holes and if you used them in a particular way you could predict eclipses, and there were various alignments, not just of the rising of the midsummer sun, but of various standing points in the moon's course through the sky. Fred Hoyle, the very distinguished cosmologist, got into the field and showed how Stonehenge could have been used in a different way, to compute predictions of the movements of the sun and moon, and maybe also of the stars.

Hawkes and Atkinson both took a scathing view of this, saying that we were simply reading our own preoccupations into Stonehenge. It was Jacquetta Hawkes who said that every age has the Stonehenge it desires and deserves, which was a very good remark and is exactly what you're saying, that we project our own preoccupations onto Stonehenge. You could indeed take that as the world of archaeology in microcosm. And that is indeed what some of the postmodern



archaeologists argue. They call themselves "postprocessual," and I always try and refute the supposed sequential periodicities that that might imply, but certainly they take postmodern arguments to say that all the readings and interpretations are ultimately in large measure subjective, and they certainly refute the view that you can have any kind of objective assessment of the data. They argue that data are never objective because you have preoccupations when you gather them and so on. But I think many archaeologists, including myself, still regard objectivity as an appropriate goal, even if it's a goal we cannot expect to attain—it's a different kind of Holy Grail.

Moreover, I think many of us would take and indeed have always taken what you could regard as a realist view of the past. Part of the critique of the New Archaeology was that in some ways it was irredeemably positivist and indeed that it embraced a positivist philosophy of science. But if you think about it more carefully, philosophers would distinguish between a realist view and a positivist view. The realist view would imply that most of us do imagine there really was a past, that we are finding material records of events that really did happen, and that they must have happened in a particular way, and in some senses in only one way, given that there must have been a sequence of events. It may be difficult for us to know that, and it may well be that somebody living at the time would have viewed and interpreted those events in different ways, but nonetheless, the events really did happen. Well, that is not a positivist view, and I don't think any of the New Archaeologists would

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for a moment have disagreed with that, and, if I am right, the critique of the New Archaeology as being totally positivist in some sort of precise philosophical sense is not an appropriate one.

To go back to your point, if one takes a realist view, then it's perfectly true that we could all have different explanations for the events which took place. We could have different views of which events were significant and which were not significant, and we could have different interpretations about their significance. But nonetheless, I would hold that it is not inappropriate to imagine that there was a past which really did happen. I'm not sure that we can demonstrate that, so it is therefore in some senses a tenet of belief. But it's not one that in itself involves a very great degree of subjective interpretation. Maybe that charge can be levelled at us a little further down the line. There, already, I think we do have some sort of divide between those of us who say, "Right, the past really happened, now what can we proceed to say about it in as objective a manner as possible," and those of a more subjective mode who argue that our interpretations and the facts are not objective anyway.

Sometimes we're not getting too far away from the view that we can start to set the facts aside, and this is a charge I've leveled at some of my professional colleagues who embrace postmodern modes of thought. Sometimes their position is very difficult to distinguish from the position of UFO enthusiasts, or the people who believe in what Jeremy Sabloff and others have criticized as pseudoscience. In fact,



when I have pointed this out, they have been very angry and said, "How could you make such absurd allegations?" But they've never been able to show how their position, which asserts that the individual has the right to choose the evidence that he is going to follow in forming his view of the past, differs from that of somebody who would believe in ley lines. Do you know about ley lines?

SMITH: No.

RENFREW: Well, in the twenties and thirties there were people who noticed that if you looked at prehistoric monuments on the map and studied their position carefully, they seemed to arrange themselves along straight lines, and if you drew these lines on the map, you would find that this applied not only to prehistoric monuments but also to parish churches, for instance, and ancient cathedrals. And sometimes pubs, or even telephone boxes were found on the same lines. So these people began to be persuaded that there were these lines of great significance which were established already in a remote prehistoric period, and then significant buildings were constructed along these lines so that in some subtle way the memory of the lines has persisted.

[Tape VII, Side Two]

RENFREW: There are people who are very much impressed with this, who spend their weekends exploring ley lines and looking for further evidence of such phenomena. I think it's a complete misapprehension, to put it at its most polite. I once took part in a television program where a lot of these enthusiasts were gathered

1. The first part of the document discusses the importance of maintaining accurate records of all transactions and activities. It emphasizes the need for transparency and accountability in financial reporting.

2. The second part of the document outlines the various methods and techniques used to collect and analyze data. It includes a detailed description of the experimental procedures and the statistical analysis performed.

3. The third part of the document presents the results of the study. It includes a series of tables and graphs that illustrate the findings of the research. The data shows a clear trend of increasing activity over time.

4. The fourth part of the document discusses the implications of the findings. It suggests that the results have significant implications for the field of study and may lead to further research in this area.

5. The fifth part of the document concludes the study. It summarizes the main findings and provides a final statement on the importance of the research.

together, and it was less easy to say to their face that they were clearly all totally mad, because to meet them, such was not the case at all. They spent their holidays establishing these ley lines, and it's true that if you don't insist on precise geometrical, laser-beam precision, things often do fall roughly in a straight line. Ultimately, the only question is, does this happen at a greater frequency than probabilistic theory would allow for? The answer seems to be, very often not. People will say, "Look, it isn't just three or four. We've got seven or eight things all in a straight line. Now, surely that goes beyond normal probability!" But to me, and to most archaeologists, the views of these people are . . . to say "manifestly absurd" is perhaps putting it discourteously . . . "are not well founded" is a better way to put it.

But then, you see, I say to my postmodern colleagues, "Okay, if you're proclaiming subjectivity, how do your procedures differ from the procedures of those who go through the countryside looking for ley lines? How do you distinguish? About the only writing I've done criticizing the postmodern approach was an article in the *Norwegian Archaeological Review*, and I set all this out concisely. People like [Michael] Shanks and [Christopher] Tilley were very irritated and said this was an irresponsible way of approaching the matter, but they never quite got around to explaining how they would methodologically distinguish themselves from these fraternities, and indeed sororities, of New Age persons, who also talk about the influence of the stars, astrology, the signs of the zodiac and so on. How do you



distinguish serious inquiry from what I think may reasonably be termed pseudoscience? This is a question that they have not seen fit to answer. Now that may be a rather extensive reply to your innocent question, but it does lead us into interesting areas in contemporary archaeology.

SMITH: Yes. So perhaps what you are saying is, you were guided by a myth of objectivity, which has heuristic value for you?

RENFREW: Yes.

SMITH: And requires certain practices of you?

RENFREW: I think I haven't actually put it that way myself, but since I accept that it isn't an attainable goal, I think that's probably a very fair way of putting it. I think that's right.

SMITH: But part of your test of the objectivity is that you have three disciplines right now that you can draw upon, which have developed autonomously, where you can say the evidence points in the same direction, even though the basis of your argument initially was purely archaeological. However, as I recall, in 1979 you published an article in which you discredit the likelihood of finding a proto- Indo-European root language, or an Indo-European homeland.

RENFREW: Which article was that?

SMITH: Let's see . . . I'll have to go back through my notes.

RENFREW: While you're looking, let me just make one point before we lose track of



it: I'm not claiming at all that the archaeological evidence, the linguistic evidence, and the genetic evidence give three independent approaches, all of which individually give clear-cut answers which converge in a satisfactory manner so that the validity of the hypothesis in terms of demic-diffusion—if we regard that as the archaeology—is securely confirmed by linguistic and genetic arguments. Not at all. I'm not saying that there's anything clear-cut like that. I am saying that there are different spheres of operation, and if you try and bring the archaeology to operate in interaction with the linguistic arguments, then it is the case that the genetics can give you insights which do have an independent status. But since the genetic data are being differently interpreted at the moment, I'm not in some complacent way saying, "Look, I have three independent disciplines which are converging, so there we are." I'm not making so bold a claim, because I don't think it's valid. Nor indeed do I feel that the arguments in favor of my hypothesis are entirely satisfactory. I accept some of the criticisms that have been made by the people who say we have to pay more attention to the preexisting populations. Despite that, I still think that the demic-diffusion model has enough going for it; it doesn't have to be a total replacement of population.

With regard to linguistics, the hope would have been that once one had proclaimed that this was how it was in terms of a realist image of the past, finding the sequences of the spread of proto-Indo-European from a homeland in Anatolia ideally would lead linguists to say, "Ah, now I can see better how certain details of the



linguistic picture work out decisively better than hitherto." And that really hasn't happened. It certainly doesn't work at all with the Gimbutas model either, which is really the Childe model, of migrations of horse riders from the Ukraine at the beginning of the bronze age. I think there are all kinds of reasons why that is wrong. My original position was essentially that we have some misconceived notions about the European bronze age and iron age, which are based on a migrationist hypothesis of horse-riding Indo-Europeans at the beginning of the bronze age. I think one can show that that is erroneous in quite a number of ways. I just don't believe there was any such sequence of events, but people say, "Well, you still have the Indo-European phenomenon, haven't you, so how do you explain it?"

A hypothesis is not refuted simply by reference to the data; you have to have an alternative theory. So that then led me to produce one, which I still think, as a first-order explanation, is probably broadly correct, but there's much more to say, and you need all kinds of second-order explanations. After all, if you're talking about events some seven or eight thousand years ago, things have also happened since, in different areas in different ways. Had the theory been just plain right, it should have led the way to deeper insights in the linguistics, which it clearly hasn't done yet, although there are beginning to be indications that it might do so. For instance, if you are looking for linguistic predictions from the model or from the hypothesis, if the place where there were Indo-European speakers first was Anatolia, and then, by



demic-diffusion, farming and proto-Indo-European speech came to Greece and then spread through Europe, if you ask which later branch of Indo-European languages would differ most from the others, it would be Anatolian, obviously, because that was the one that was left behind first.

It's interesting that at a conference in Philadelphia recently, Donald [A.] Ringe, who is one of the very few linguists at the moment trying to use quantitative measures of similarity and difference, announced very firmly that in his studies he finds robustly repeated the notion that the Anatolian branch of Indo-European is the most different from the others, and in that sense it was the first to split off. Well, that is a specific prediction of my model. It is not a specific prediction of the Gimbutas model. In the Gimbutas model there was nothing preferential about the Anatolian area or the Anatolian languages; in my model there is. So there are some indications that some aspects of my hypothesis are actually working out with linguistics; nonetheless, one would have hoped for more. If I'm right, there are clearly other strong second-order effects which are coming into play, masking the simplicity of the first-order explanation, which isn't surprising, considering the great time that's involved.

SMITH: Yes. There's a whole batch of criticism which we could lump together as "localist." They come up with a whole series of individual exceptions.

RENFREW: I'm just trying to visualize exactly what you're intending by "localist" in



this context.

SMITH: Questions such as you've already mentioned, of overlap of farming communities with hunter-gatherer communities, the questions, from the linguistics point of view, of how one understands glottochronology and—

RENFREW: What did you mean by "localist" though in this context?

SMITH: Even though your model may explain the aggregate result, there are many, many particulars that then need second-order explanations.

RENFREW: Oh, but I think that's absolutely right. I would say that myself. It's very difficult to find any correlations at all between archaeology and language, in some senses, so I haven't sat down to sketch more detailed scenarios, but I think that statement is absolutely right. Indeed, you have to discuss how the different language families within Indo-European developed, what processes were involved. Some are easier than others. One imagines the Italic languages developing in Italy, and then the Romance languages spread with the Romans, but when you talk about the origins of the Celtic languages, you would have to ask why they differentiated from the Germanic languages. Then you have the Slav languages and their place of origin has always been open to question; and the Baltic languages: Lithuanian and Latvian. So there are lots of issues that need to be further explained, that is right.

SMITH: I actually wanted to ask you if it was important that the model be real, that it have an ontic validity. I mean, for instance, your work with geographic plotting and



catastrophe theory did not require a claim for the reality of the findings. What you were doing was providing an equally plausible way of looking at the data that would then complicate previous claims.

RENFREW: I'm not sure if I'm happy with that. We should perhaps look at the two cases. First of all, in the catastrophe theory case you were speaking of, I was arguing that in this way we could see how discontinuous change can be produced through the continuous operation of causal variables, but I don't see why that should be non-ontic and lacking in reality. I mean, if you have an equation in the sciences, say, something very simple like the refraction light, the equation is an abstraction which describes the first-order reality, and the first-order reality may well very satisfactorily account for the bulk of what you observe, but in the real world you have other effects, so it's always the situation of other things being equal.

Now, in human affairs that is also certainly so. There are cases where you can apply equations in a fairly noncontroversial way: you have exponential growth of populations, and then a better description is logistic growth. You can certainly find plenty of cases where if you look at specific human populations through time the pattern has been one of logistic growth. Well, that's only an equation, and it's still true that individuals were living and talking and dying and behaving in nonstatistical ways and so on, but nonetheless, in aggregate, that is a good first-order description of what happened. Then you find that in detail it doesn't work in various ways, and of



course there are other things that you have to explain taking place . . . but would you call that a non-ontic model?

SMITH: Well, not necessarily. If you can relate the secondary phenomena to the overall conclusion, then you would be able to begin making an argument for the ontic reality, but right now—

RENFREW: I would need to understand a little better what you intend by "ontic reality."

SMITH: Well, actually, that's a redundancy, but—

RENFREW: I understand ontic as *being*, so you're saying a "real reality ." Now, what do you mean by a "real reality"?

SMITH: Well, did you intend to describe something that you believe actually happened, or were you putting forward a model that could explain the end result but makes no necessary claims about whether the model in fact corresponds to what the processes were that led to the end result?

RENFREW: I'm not sure that I entirely see all the distinctions. If we take the catastrophe theory case, to start with, and we apply that to a specific case, like the Aegean bronze age, it is perfectly possible that there actually was a cause of a discontinuous nature. For instance, if you had invading Dorians coming in, which was for a while the standard view, destroying the preexisting civilization and setting up shop later on in their own style, then that would be a different kind of explanation,



because it actually is offering you a discontinuous cause which might explain very satisfactorily the discontinuous effects. Now, I'm not saying that I know what happened, or that anybody does; it's clear that there must have been a whole series of variables, but one of the nice things about catastrophe theory is that it allows you to say that and to believe that. So I'm offering an explanation in the general form, and then you can go on to suggest various possible scenarios as how it might have happened.

I don't really see how I could put myself in the position to say what did happen unless I had much more abundant data that led me to more specific hypotheses. And I don't really see how I could claim those more detailed perceptions without being like Jacquetta Hawkes, who at times claims anamnesis: unforgetfulness. She actually was there, she claims, and saw it all, and she's woken up and remembered her previous existence. Well, if you can employ your anamnesis, then you have your special insights into reality. It may be though that in discussing the catastrophe theory case, which does allow a lot of imprecision about the specific variables, we're not able to argue your point. You mentioned the location analysis case, and that might give us room for more concrete discussion.

SMITH: Actually, part of what I'm trying to get at, and perhaps unfairly, are aspects of the philosophy of science, and how you as an archaeologist do your modeling, and your expectations. There are a couple of famous examples. The model of black holes



was developed purely as an imaginary construction, a mathematical game. Not only was there no claim that it had a reality, but the Polish mathematician who modeled it was convinced that it *couldn't* exist in reality. Then fifty years later these mathematical games that he had developed proved to be useful for describing phenomena that astronomers were observing.

RENFREW: Right. Certainly in physics you find many things like that, where quantum equations have thrown up specific results that ultimately would predict a new fundamental particle, and then, lo and behold, a fundamental particle is indeed found with those particularities. This, in a way, relates to the general issue of the reality of models, so you're right, it is a discussion in the philosophy of science. Funnily enough, that was one of the things I touched on in that paper I wrote as an undergraduate, on the existence of theoretical entities. When can you feel that a theoretical entity exists? After all, electrons and atoms were predicted before anybody could claim they were observed. Now I think most people imagine that atoms really exist, but they did start off as theoretical entities. So you're quite right, there are problems associated with determining what it is that gives you the feeling that theoretical entities really exist. With atoms of course we imagine them as little billiard balls or something, which are palpable, so it's easier to think of atoms as having a real existence than some of these very strange fundamental particles which don't quite partake of the same nature.



Perhaps our discussion about the reality of entities or explanations is similar.

In fact, I asked Hugh Mellor, who is a philosopher, to contribute a paper for that Sheffield symposium we were speaking of, and he wrote an article on the existence of cultures as theoretical entities. He ultimately took a position that if they are convenient to think about and are helpful in explanation, you are as entitled to think of them as real as you are to think of atoms and electrons as real. Now, I'm not meaning to defuse your question into a whole cloud of different philosophic argumentations; perhaps we should return to it again in the light of those observations—

SMITH: Part of my question is, what work were you trying to accomplish by proposing the model? Obviously, you had reasons for locating the proto-Indo-European homeland in Anatolia rather than elsewhere—

RENFREW: I didn't, actually. I'm not directly contradicting you, but it can be put a different way. The approach which I was following *itself* led to the conclusion that it would have to be Anatolia that was the starting place. More recently, I assembled the arguments that horses were only ridden with military effect and with military purpose much later, around the beginning of the iron age in Europe, around 1000 B.C. Until around that time there is absolutely no evidence that horses were ridden to military purpose. There are quite good arguments to support this position. The fact that horsemen are not represented in drawings until later I think shows they were not of

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interest until later, or maybe did not exist until later. So, if we say we are totally unimpressed by these Kurgan invasions and we simply do not believe they happened, and if we take the explanation of the Beaker culture, which was part of Marija Gimbutas's argument, as being of a different nature altogether and perhaps difficult to understand, then, no, the explanation was not proto-Indo-Europeans coming in from the east. A number of archaeologists were skeptical of all that. But then you say, "But wait a moment. We have these languages, which are classified by linguists as Indo-European, right across the distribution where they are found. Have the linguists got it wrong? Is this some illusory classification? Is it a classification with no real meaning?"—to go back to your notion of reality. Well, linguists very widely agree that the relationships are such that they must derive from a prototype; in other words, it's a family tree-type model: as you go back up the tree you converge to a specific ancestral language.

There was a linguist called [Nikolai S.] Trubetskoy, who did propose a different model about which linguists get irrationally angry. Once at a conference I mentioned Trubetskoy, and the very distinguished linguist, [Aron] Dolgopolsky stood up and said, "You should not speak of this hooligan Trubetskoy!" [laughter] It later transpired that this colloquialism was not precisely the one he really had wished to choose, but he disapproved of Trubetskoy. Anyway, if you have some notion that there must have been a proto-Indo-European group, which must be ancestral to later



speakers of Indo-European, you are looking for some unifying process, really, and it's clear to me that there just is not evidence to suggest there was a migration in the bronze age so large as to have a unifying effect on the whole of Europe.

If you look at European prehistory, you could take it back to the paleolithic and you might say that maybe the first humans, forty thousand years ago, were speaking proto-Indo-European. Archaeologically, you could certainly use that argument, and more recently some archaeologists have begun to say that, but that puts the historical linguists into complete despair, because they have their time frame, and they say, "We know that proto-Indo-Europeans couldn't have been around more than three or four thousand, or at the most five thousand years ago." They know it, but I've never found anybody who could really explain satisfactorily how they know it. I totally disbelieve their chronologies, which I think are of a circular nature, but anyway, that's what they tell you. If they're not allowing you to have proto-Indo-Europeans as long ago as 7000 or 8000 B.C., they're certainly not going to allow you to have your proto-Indo-Europeans forty thousand years ago, so probably the paleolithic explanation may not be the right one to go for, though it is a possibility.

If you ask yourself what process or series of events in European prehistory were so radical as to have an impact on the whole of Europe, the only obvious answer is the origins of farming. Then if you take demic-diffusion as a good model of the origins of farming in Europe, you can say, "Ah well, yes, this is the only process so



radical that it could explain the Indo-European languages." You can then say, "Okay, if we're using that model to explain the Indo-European languages, what does that tell us?" And out of the decision to use that model comes the where and the when. If you decide yes, this looks like the explanation, then *therefore* Anatolia would be the homeland, and *therefore*, given that we have good chronology for the spread of farming, the origins of farming in Anatolia would be 7000 B.C., I suppose, on a calibrated timescale. Sometimes people misunderstand this point. I'm not particularly advocating Anatolia. It is the case that Anatolia is where farming came to Europe from, so if you are embracing the model, that means that in most arguments your Indo-European homeland will be in Anatolia and it will be at that time.

You need some fundamental process that's going to be so extensive as to explain so widespread a phenomenon as the Indo-European language distribution, and the only fundamental process that seems remotely plausible as an explanation is the spread of farming. So how do we model the spread of farming? Demic-diffusion may not be ideal, but it's basically the right first-order model. The plant and animal domesticates came from Anatolia, so farming in that sense spread from Anatolia, so if you are looking for a homeland, then it's got to be Anatolia, around 6000 or 7000 B.C. Then you begin to ask, "Well now, how do we make the rest of it fit?" You then see that indeed the steppe economy did develop in the Ukraine, sometime in the fifth millennium, and that's when horses were first intensively exploited, but for food, not



for military purposes. And then you say, "What about the eastern branch, Iran and India, and the Tocharian languages?" Strangely enough, Marija Gimbutas and Gordon Childe never wrote much about that side of things, but there I think their idea was right, that it was the development of the steppe economy, based on nomad pastoralism. Again, it was a kind of demic-diffusion, but of a rather different kind, based on that new economy of nomad pastoralism. It may be that there was an initial spread of farming economy by the first proto-Indo-Europeans into the steppes, but then you do have a development of nomad pastoralism by Indo-European speaking groups, and that allows you to begin to discuss what happened in India and Iran.

Jim [James P.] Mallory wrote a good book, *In Search of the Indo-Europeans*, which was essentially reiterating the traditional view and bringing it up to date with archaeological observations. He and I would probably very largely agree about the Indo-Iranian side of things, because we both agree that essentially the development of nomad steppe pastoralism was at the origin of the eastern Indo-European spread. There isn't really much divergence of view there. Of course that is where the compromise view begins to emerge. Cavalli-Sforza rather rides with the hare and with the hounds as concerns the Indo-European question, because sometimes he likes to say that his demic-diffusion model accounts for the spread of Indo-Europeans, but at one point he made what I think was a mistake in his explanation of the three principal components for the genetic map of Europe which his analysis yielded. The

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first he identified as the farming spread, but principal component three comes from east to west, so he said, "Oh well, maybe that's Marija Gimbutas's Kurgans," so he's rather embraced that, though he sometimes moves away from it. But he and others have also said there may have been an original spread from Anatolia of early proto-Indo-Europeans. As the demic-diffusion model would express, the early farmers do indeed come to the Ukraine from the west, and a steppe farming economy develops there, which is for that reason Indo-European, and then maybe there is then a secondary spread. Well, our views are converging quite closely there, particularly since we would agree for the eastern part, but despite that, I think there is very little evidence for the whole Kurgan business at all, except maybe in Bulgaria, Romania, and a little way into Hungary. So I'm not impressed with that argument. We could, if you wish, talk about the third (east-west) principal component, but you may have other things you wish to discuss.

SMITH: No, why don't you go into that.

RENFREW: Well, over the centuries and millennia there have indeed been east-west interactions, and when you are working with principal components, each one is a palimpsest of all that has ever happened, so one principal component can be analyzed in this direction, another in this direction, another in this direction. Each of them is a palimpsest of all that has ever happened with that directionality, so that if you have later movements from Anatolia to Europe, or indeed the opposite, they too form part

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of the first principal component. We know that the Huns and the Mongols, for instance, were influential in the first millennium A.D., and we know the Scythians and the Sarmatians had their effects in Europe in the first millennium B.C., so the third principal component would be the palimpsest of all these things, and it may well be that very little of significance of that kind was happening in the bronze age.

SMITH: This question concerns the Nostratic hypothesis. You then took a step back, to look at how the proto-Indo-European connects to proto-Dravidian and proto-Hamito-Semitic. Why did you feel that would be a beneficial step to take?

RENFREW: One of the reviewers of my book *Archaeology and Language* said, "Oh dear, oh dear, Professor Renfrew is totally ignorant of the Nostratic hypothesis."

Well, in 1987 that was an extremely accurate remark. I think the first time I heard of the Nostratic hypothesis was in that review. I forget the name of the reviewer, he was one of those contributing to the *Current Anthropology* critique, but that review was spot on. So that did lead me to think that perhaps I ought to find out about this Nostratic hypothesis. It indeed came my way in other senses, because one of the best examples of demic-diffusion comparable to the proto-Indo-European spread, is the spread of the Bantu languages in Africa. So I began to read a little into African linguistics, and the great towering figure in African linguistics is Joseph Greenberg. He sat down many years ago to classify all the languages of Africa, and he classified them into just four major language families. Most of the African linguists at the time

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said it was very doubtful, and oversimplified and so on, but, nonetheless, that classification has since then become very widely accepted; it's now the standard classification that most linguists follow. Many believe that it has some degree of historical or ontic validity and isn't just a mere classificatory device, as it were.

So I began reading in this direction. Merritt Ruhlen wrote an excellent book on the languages of the world. I had looked at earlier books which classified all the world's languages, but they were really a terrible muddle to get through. Ruhlen was following Greenberg's classifications for Africa, and he also followed them for America, which of course had been hugely controversial. Those linguists who accepted the classifications for Africa were outraged at the temerity of Greenberg to achieve such a gross oversimplification for the languages of the Americas. That's always interesting, you know; it's nice to see good, simple conclusions. If you can find first-order simplifications, that seems a good thing to do, even a scientific thing to do, if I dare say that in this sort of postscientific era. Greenberg is one of those who talks about macrofamilies, big families.

In the course of reading I came on the other group of scholars who talk about big language families, and that is the Russian school, initiated by Ilich Svitych, and I think independently by Aron Dolgopolsky. They speak, as you say, of a Nostratic family, and they use quite traditional linguistic methods to show similarities. They don't just compare vocabulary over a wide area, they are willing to undertake



linguistic reconstructions. Much of the criticism of Greenberg has been that he rushes at it, he doesn't do the linguistic reconstructions—he "hasn't done his homework" is basically the grading that he gets from some fellow linguists. Whereas the Nostratic people had done their homework and were willing to offer prototype forms. They see the Nostratic macrofamily as not only involving Indo-European, but Dravidian, Altaic, Afro-Asiatic, and the Kartvelian languages; they put them all together.

I really have no idea at all whether that is a valid classification, and I am impressed that so many linguists say this is rubbish. While I accept the consensus among linguists that there is an Indo-European language family, it's very difficult to know which way to assess this Nostratic hypothesis. But if the Nostratic theorists are right, then there would be an earlier stage when there would be proto-Nostratic spoken somewhere. It dawned on me that the demic-diffusion approach for the spread of farming to Europe could possibly be paralleled by similar effects elsewhere, because you have a nuclear area for farming in the Near East, the hilly flanks of the fertile crescent. Just as you can see Anatolia as one of the hearths, the *foyers* of early farming in Europe, so the Levant area could be seen as the starting point for the domestication of sheep and goat, and to some extent wheat and barley, and their spread to North Africa; it's a very analogous case to what you have in Europe. The same may be true for the Indian subcontinent. The neolithic site of Mehrgarh, in Pakistan, which is by far the earliest farming site in south Asia, had wheat and barley,



and sheep and goat. A lot of the Indianists try and argue that it's all local domestication: in fact, the specialists in plant remains, [Daniel] Zohary and [Maria] Hopf, say this is essentially Near Eastern stuff, wheat and barley, so it may be that there was some such spread to India. Then if you're talking about Turkmenistan, you have early farming there, and, again, the basic economy seems to be an imported one from further west, from the hilly flanks of the fertile crescent.

[Tape VIII, Side One]

RENFREW: It was in that nuclear area that farming originated, the hilly flanks of the fertile crescent, including Anatolia. If you accept any notional reality for the Nostratic macrofamily, and you are looking for an earlier protolanguage, then that's the area that you would look for. You would have absolutely the same mechanism at work, though it may vary in practice; that's where you have the model with the four lobes, which was first drawn out by Andrew and Susan Sherratt in an article in *Antiquity*. I adopted it rapidly, because it applied well, and it might possibly allow one to see a historical reality behind the Nostratic macrofamily. I now have a friend, Guido Barbujani, a very sophisticated statistician, who has analyzed the genetic evidence and has found some support for this Nostratic hypothesis from modern gene frequency distributions across these areas.

It is very interesting to think of all the linguists who totally decried Greenberg for his temerity in having just three macrofamilies in the Americas, the Eskimo-Aleut,



the Na-Dene, and Amerind; they said he was crazy to lump all these languages together and call them Amerind. Yet some initial studies, which I wasn't entirely happy with, using teeth shape and so on, seem to support that. And now studies using mitochondrial DNA seem to be giving very strong support to the notion that Amerind speakers are in general descended in lineages which are not the same as those of the Na-Dene and the Eskimo-Aleut.

In parts of Central and South America, [Antonio] Torroni and his colleagues have found modern speech communities who actually differ genetically quite markedly from neighboring speech communities, and there's a very strong correlation between the linguistic affiliations and the genetic affiliations. So on first evaluation of the genetic evidence, it really looks as if Greenberg is getting strong support from the geneticists. This hasn't been so in Europe, interestingly; genetic evidence isn't working in such a clear-cut way, and I can't claim it's supporting my arguments clearly, but certainly in the Americas the mitochondrial DNA supports Greenberg.

One of the things I've really found fun about all this is having the opportunity to look at things on a global scale. Of course, I have no knowledge of linguistics on a global scale, nor indeed of genetics, but then nor have most linguists, so I think one may cautiously enter the field, and that is where I have felt able to generalize that the issue of farming dispersals may be a very important one for many of the world's large language families. So it may well be legitimate to talk about macrofamilies in the way



that Greenberg or the Russian Nostratic school do, and therefore, it may be that one isn't totally wasting one's time in rather speculative hypotheses that are possibly just sheer nonsense, as most linguists would regard them. Because if Greenberg's approach is accepted in Africa, if it's accepted in the Americas, then to speak about macrofamilies in Eurasia may also be reasonably valid. If one accepts that there may be real ontic entities there, which perhaps is a reasonable view, then maybe their distribution is worth explaining, so maybe the farming dispersal model turns out to have some validity there.

SMITH: Well, if a proto-Nostratic or Nostratic population develops farming in Anatolia and then spreads out in the four areas, don't you at the same time dissolve proto-Indo-European?

RENFREW: No, Anatolia is only one segment of the area in which early farming developed. It developed earliest in the Levant. If you wanted a single central point of diffusion of farming it might well be the Levant, but it's also happening in the Zagros mountains, and it's also happening very early in Turkmenia. Given that these are different language groups, if you want to make these things tie up together, you have to assume that around 8000 B.C. you have proto-Indo-Europeans in Anatolia, proto-Afro-Asiatics in the Levant, proto-Elamo-Dravidians in the Zagros, and proto-Altaic or something in Turkmenia. Then if you accept that the demic-diffusion model in each of those areas is plausible, you have to say that those populations all appear



contiguous: we're describing ancient western Asia, really. So they had differentiated into proto-Indo-European and proto-Elamo-Dravidian, or what have you, by 8000 B.C., and then perhaps they did have a common origin much earlier, say 15,000 or 18,000 B.C. We're beginning to talk, therefore, about the upper paleolithic populations of those areas, and maybe they had some commonality some thousands of years earlier. This hasn't been spelled out in detail. One would need to be looking at developments in the late paleolithic which might allow you to speak in terms of a single, more unified population for the proto-Nostratic—maybe 15,000 B.C.

SMITH: One follow-up question on an aspect of demic-diffusion, which has to do with the question of warrior societies. What made you doubt that the initial societies were warrior societies? Why were you so firmly convinced that that aspect of the Indo-European story was wrong?

RENFREW: You mean the societies in the Ukraine, where Gimbutas or Childe would have them come from?

SMITH: Right.

RENFREW: They may have been fighters to some extent, but there's no particular archaeological evidence for it at that time. In particular, I think the real nonsense that is talked is about the horse, although in recent years more evidence has come to light relating to the early use of the horse in the Ukraine. At a site called Dereivka, there is a high proportion of horse bones in the animal remains, in fact, such a high

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proportion—I think more than 70 percent—that it's perfectly clear that the horses were being eaten. There's no really good evidence that they were being ridden.

David Anthony is a scholar who has found one or two horse teeth which he claims have tooth wear that might suggest the use of a bit of some kind.

It's really quite clear that the horse doesn't seem to have been used in any very striking way until it was used to pull chariots. It may have been used at an earlier stage to pull four-wheel carts, though oxen were more commonly used. There are chariot burials from around 2000 B.C. in the steppes and you see chariots on reliefs at Mycenae around 1600 B.C.; it was a little earlier than that in the Near East. So the chariot comes into being in a big way, and there you see persons of prestige riding in the chariot with swords, and you have a whole military complex. What I realized when I was looking into this earlier this year was that wherever you have evidence of the chariot being used, you also find representations. You have stelae, or rock engravings, or little figurines. In other words, once the chariot comes in as an instrument of prestige, then it's reflected in other ways; it forms a key theme in the iconography. Without exception, ridden horses are a key theme in the iconography five hundred years later, in each area. You begin to see warriors on horseback in the iconography from around 1000 B.C., usually rather later, and you also begin to get archaeological evidence for this. You don't find many metal horse bits prior to that time, and the horse bits that you do find earlier probably were in relation to horses



pulling chariots.

So I think it's rather persuasive that ridden horses were not of much significance at the time chariots were first used, or they would have shown up in the iconography along with the chariots. We have no evidence that horses were prestigious or of any great significance earlier. There were one or two horse burials, and they may have had some ritual significance, but there is no evidence at all that they were ridden by warriors. Now, the converse is often asserted: it's often said that the Ukraine was important, that's where the Indo-Europeans came from, and they were mounted warriors on horseback. If they were mounted warriors on horseback they didn't come till about, say, 1200 B.C. at the earliest, and that of course does not fit in with the Childe-Gimbutas model at all. You could well say, "Well, weren't they warriors on foot?" Well, they very possibly were; in the early days you don't have very much in the way of daggers, later on there may be a few, but there's no particular evidence to suggest they were more warlike than anybody else. Even in the neolithic you have evidence for fighting sometimes, and fortifications in some areas, so I've no doubt that very early on people had hostilities and attacked and defended themselves, but this story about the inhabitants of the Ukraine being especially warlike in the early days is a total myth.

You don't really see much grandeur about warfare until the early bronze age, and you actually see it as clearly in northwestern Europe as anywhere else, where you



have what you can describe as warrior graves. Then later on you get warrior graves with chariots, as I've been describing, and then you also get warrior graves with horse harnesses and so on. So it's a very clear overall picture. With the bronze age you begin to get an emphasis on weapons, and this was as clear in western Europe as in eastern Europe, and I think as early. There were daggers first of all, and then more impressive weapons appeared. In the late neolithic cemetery at Varna, in Bulgaria, which you mentioned [off-tape], you do have some weapons of prestige, but this was before the dagger was introduced. So there was just nothing particularly warlike going on. What you do seem to see is the evolution in the bronze age of chiefdoms, and then coming into the iron age you have persons of very high prestige, and it's clear to me that these features were typical of the Indo-European societies, including the Indian ones in the late bronze age and iron age, but they were not typical of *early* Indo-European societies. There were social features which indeed evolved in different parts of Europe. I would say that they evolved independently, not because they were Indo-European features, but because of the processes at work in Europe and indeed beyond.

So I think all this war-like stuff as being characteristic of the Indo-Europeans is a complete myth. It is true that the European bronze age was a time when interest in weapons and the panoply of war emerged, so all that is perfectly reasonable, but there's no particular reason to associate it with the Ukraine; the only reason to do so



is the misapprehension that the horse was a significant military instrument, which it wasn't until the horse and chariot had come into use, as I just indicated.

SMITH: You indicated earlier that the genetic evidence in Europe is not quite as neat in supporting the language hypothesis as it has been in the Americas. How has that affected your basic propositions?

RENFREW: Well, there is some support coming from the classical genetic markers which Luca Cavalli-Sforza and his colleagues analyzed. Their conclusions strongly supported the demic-diffusion hypothesis of farming from Anatolia, which does not in itself say anything at all about language, but on the other hand, a very important component of my story is demic-diffusion from Anatolia. Then I mentioned Guido Barbujani, whose analysis of the genetic evidence showed some support for a broader Nostratic viewpoint. One might also hope that mitochondrial DNA would give support as it has done in the Americas. But when you look at the lineages of mitochondrial DNA, you find you seem to be at a greater time depth, so you are presumably looking mainly at what was happening in the paleolithic and there is no very clear patterning that can be related so far to this neolithic demic-diffusion process. That doesn't necessarily mean it's evidence against it. For instance, if the population in Anatolia was genetically rather similar to the existing population in Europe from the standpoint of mitochondrial DNA, a spread of population from Anatolia to Europe wouldn't particularly show up. So I think it's for the geneticists to



clarify what they can about the population history of Europe, and it's also for the geneticists to elucidate for us why you get very clear patterning when you are talking about the first principal component, using a whole mass of genetic markers, but when you look at the mitochondrial DNA you don't see the same patterning. Mitochondrial DNA, for one thing, may represent a different time depth, and change may be produced in different ways, so it may be illustrating a different facet, I suppose, of the population history. But I don't understand that and I haven't heard any geneticists clarifying it very well.

It's only about a year since the mitochondrial DNA has become very clear for Europe, and the whole issue will become clearer when the Y chromosome DNA is investigated, which is now technically possible with roughly the same degree of facility as the mitochondrial DNA. The Y chromosome is associated with the male descent line, the mitochondrial DNA with the female descent line, so it will be interesting to see how that matches up in the Americas and in other parts of the world. The genetic evidence is mainly from living populations today, and it's accumulating very rapidly, so if we hold this conversation again in five years time there will be much more to say about the genetics. There may not be much more to say about the archaeology, and there probably won't be much more to say about the linguistics, because it's not clear where you would get new information from.

SMITH: In 1972 you went to Southampton. Did your teaching program shift at all



because of your move?

RENFREW: To some extent. The basic notions of European archaeology and prehistory were similar to those at Sheffield. It was a broader course chronologically. The Anglo-Saxon period was also taught, but it was taught by people who were already there, so I didn't have to start teaching Anglo-Saxon archaeology. There was a greater emphasis on the archaeology of southern Britain, which was fun to look into, and it was perhaps a stimulus to look at the archaeology of the Wessex area, so that when I was writing more about megaliths, all of that worked in very harmoniously. So the move didn't really make a great deal of difference to my research, I think I can say. We did develop theory more. We set up some M.Phil courses in archaeological method, so that perhaps was an encouragement to lay more emphasis on theoretical questions.

SMITH: This may not be fair, but I was wondering if you could remember what the primary texts might have been that you assigned in the mid-seventies, as compared to what you might be assigning today?

RENFREW: Yes, one can think about that a little. There have never been very good standard books, really, in recent years. At that time one would have been referring to Childe's *The Dawn of European Civilisation* very extensively, and Stuart Piggott's book, *Ancient Europe* was and remains a very useful compendium. Even though radiocarbon data have changed, it's still a very useful book. So from the European



point of view those would be standard works. Today, my own book *Before Civilisation*, in relation to Europe, would be relevant. There is a work edited by Tim [C.] Champion and Clive Gamble and others at Southampton, produced after I left, which is an overview of European prehistory, and that would be quite a useful text. In terms of method, you really had to find a book in each area. I think for the history of archaeology, Glyn Daniel's *The Idea of Prehistory* was a first-class work.

For archaeological theory there really wasn't very much. Gordon Childe's book, *Piecing Together the Past*, gave the Childean viewpoint, but mainly there it would have to be articles. Certainly there was Binford's *New Perspectives in Archaeology*, and also David Clarke's *Analytical Archaeology*, though that's fairly heavy going. Today I'm not sure there's all that much more. It's true that the handbook which I wrote with Paul Bahn [*Archaeology: Theories, Methods, and Practice*] covers many approaches to archaeological methods, so that would be used as a beginner's text today in most university courses, and I would certainly refer to that. It's always been a problem to get really satisfactory archaeological textbooks, and I'm not sure that there are very many even now, in this country or in the United States.

SMITH: What about Grahame Clark's handbook on prehistoric man? It's about so thick and it covers global prehistory.

RENFREW: That's his book *World Prehistory*, yes. That certainly is a very useful



text, but its treatment of Europe specifically is not sufficiently detailed to make it useful for the neolithic, bronze, and iron ages. One might use it for a first-year text, if you wanted to cover fairly rapidly the whole of the paleolithic; it covers that in a very good way. And then you might refer to it if you wanted to do some comparisons of world civilizations. On the other hand, if you are really focusing on the origins of civilization you'd need more about Sumer, and one would indeed refer to Childe and others for more detail on the ancient East and on Egypt. So Clark's book has not been used a great deal for university courses in this country, I think; perhaps it's used rather more in the United States, where it may just fit some people's bill. It's a very good book, and it puts all of that in one volume, but if you are treating the subject in more detail you may require a more detailed text, certainly for Europe.

SMITH: In 1981 you came back to Cambridge. How did your appointment as the Disney Professor come about?

RENFREW: Well, there is an appointing committee of electors, which is a standing committee, and they are determined years in advance, so that should there be a vacancy in the chair, the electors are already in existence, as it were. It was known in fact that Glyn Daniel would retire at his retirement age, so there was an advertisement, and I applied in the usual way. In the old days I believe it was usual for aspirants to go and call on all the electors in person, but that seemed a rather strange thing to do, so I certainly didn't do that. In those days, and indeed still, it is

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not necessary for the electors to a chair to interview candidates, but increasingly, the electors do interview candidates. I think the tradition came from when the subject was so small you knew everybody anyway, which isn't really the case now. But in this particular election there were no interviews, so I received some informal phone calls and then a letter saying I had been appointed to the Disney Chair, and did I wish to take it?

It did involve a slight reduction in salary, because Cambridge professors are all paid at the same level, and certainly if one had been in Southampton for a while and therefore was rather well established as a professor, one might hope to have a little more than the average salary. But Cambridge doesn't worry about small matters like that, they just expect people to smile and put up with it. And if you are a professor in Cambridge you expect to have a fellowship in a college, and it was quite natural then for St. John's College, where I had been an undergraduate and a research fellow, to offer a professorial fellowship, which is indeed what they did, and that was very pleasant.

SMITH: Had it been an ambition of yours to go back to Cambridge?

RENFREW: Not overwhelmingly. People sometimes used to allege, both previously and subsequently, that clearly it was my intention to aspire to the Disney Chair. Not particularly. There is no doubt that it is the oldest established chair of archaeology in this country; it was founded in 1851 by John Disney, and it is a good department with



good traditions, so it was natural to apply.

SMITH: And you had no hesitations about taking it?

RENFREW: That's right. The small reduction in salary was not all that crucial.

Cambridge is obviously a very beautiful place in many ways; it has good libraries, it really does see off most other places. Oxford has good libraries too and so does London to some extent. The college system is a very pleasant one. The department is the most ancient department in the country—not that it's all that old, it goes back to the twenties, I suppose. It has a very good museum of archaeology and anthropology. It was much worse in terms of laboratory facilities than Southampton for instance, but fortunately subsequent events have allowed one to improve on that somewhat. So, no, there was no great hesitation.

SMITH: In 1990 you became the head of the McDonald Institute [for Archaeological Research].

RENFREW: I received a letter in the department from a lawyer, who said he represented a person of some means who would be interested in visiting the department. He didn't say why, but it seemed to be implied that there might be some benefit to the department from his visit. So I and a number of colleagues received this gentleman, Dr. [Daniel McLean] McDonald, whose name was not announced in advance. He and two colleagues, one of them a lawyer, came and visited the department, rather briefly. He was a brisk, elderly sort of no-nonsense Scotsman.



We brought him back to Jesus College and gave him what we thought was rather a good lunch, and then we brought him up here into this very room and sat around and talked, and we rather expected Dr. McDonald might say something about his intentions. But he asked us a few questions about the origins of humankind and various things about different civilizations—he was very well informed about such matters—and then he said, "Yes, well, thank you very much, it was a very nice lunch." And off he went with his folks, and nothing more was heard for a while.

Then I received a telephone call from one of his colleagues asking if that colleague and the lawyer could come and call on me. I was very busy that week, so I didn't seem to have time for a meal, but I could fit them in for a cup of coffee! So they rolled up and said, "Well, it's been decided that we, on behalf of Dr. McDonald, would like to offer your department a hundred thousand pounds a year for five years. Would that be acceptable?" The money was intended mainly for fieldwork. So I indicated that that *would* be acceptable, and then they said, "Could you indicate how you might dispose of a larger sum, were it available?" I thought quickly, and being aware that premises were the great defect of the department I said I thought it would be very good to have an institute for archaeological research. I definitely put the emphasis on research, because it's not a good thing to duplicate what the university should be achieving for itself through government funding. It would be inappropriate to start paying for undergraduate courses; that would just swallow up money and

simply mean the university would put in less. Clearly, if someone is going to give you something, it has to be in addition to what you are already doing, or it's a waste of their money. So they said, "Oh that's very interesting, perhaps you could have a think about that, give us more information, and Dr. McDonald will call again in six weeks time."

I contacted the university authorities, and the secretary general moved with great alacrity and suggested that we should think of a building there in the courtyard. He encouraged me to get in touch with the director of estate management to get an architect to design some sort of building. We got a very quick design for a building from a local firm of architects. Dr. McDonald came, and we gave him a good dinner, showed him the plans, and said we'd clearly have to have this endowed to make it work. We asked the college choir to come and sing him some Christmas carols—it was that time of year—and the visit went well and he was pleased. He indicated his intention of founding an institute, rather on the lines envisaged, and the preliminary calculations suggested it might take about five million pounds to build the building, and running costs would require an endowment of another five million pounds. So that was broadly agreed, though nothing was signed or sealed at that time.

We got a number of architects to put forward ideas and grilled them, and chose the firm of Casson Conder, which was a very good firm of architects. Then it turned out that with the rather high quality of materials Dr. McDonald wanted, it



might cost six million rather than five to build the building, so that was agreed. Then we had a setback, the planning permission was refused, the planners weren't entirely satisfied with it all. So we had to think this through again, and we chose a different configuration, in fact two buildings rather than one, the one in the middle being rather smaller than previously envisaged, and that was agreed and Dr. McDonald went off contentedly. Then, very sadly, he died a week after going back to his home on the Isle of Man. But it turned out that in his will the provisions had been made, and eleven million pounds was indeed assigned to the university for these purposes.

It took a year or two to realize the funding, but it came through, the institute was founded, and we were able to go ahead and build. So it's up to us to use the building effectively and build up research programs which we are working on. While Dr. McDonald was alive, we set up a provisional institute in rented premises, and we founded the *Cambridge Archaeological Journal* with money which he originally put in separately, but now that it's going, the subscriptions pay for the greater part of the costs, and the remainder is met from the income to the institute from its endowment. So I think the thing is working really rather well, I hope very much as Dr. McDonald would have envisaged. (Certainly it is a place where all those Cambridge archaeologists in different faculties who are doing research can meet together, and it is playing a very useful role in that way.) When it was completed we invited Prince Charles, the Prince of Wales, to open the building at the formal opening and all that



went very well. So I hope Dr McDonald would have felt that what has happened would have been broadly what he wanted to happen.

SMITH: Do you know what the source of his interest in archaeology was?

RENFREW: Yes, he had a passionate interest in ancient metrology. He became fascinated in ancient measures and weights of various kinds. I don't know just how he hit on that, but he had a capacity to focus very intensely on specifics of various kinds, which no doubt was part of his success as a businessman. He had in his mind all these units of weights and measure and how they interrelated, and he wrote a series of essays on ancient metrology, which we have published as a volume. Although they are very complicated on the computational side, they raise interesting questions and reflect his very intense interest in these matters.

SMITH: Now, I believe you said in passing that it was archaeology and language that attracted him to Cambridge?

RENFREW: I believe that's so. I know that he had looked at one or two other places. He'd certainly been to the University of Edinburgh and had been interested in their department, which is indeed a very good one, but I had the impression, I suppose it must have been from Dr. McDonald's colleagues rather than directly from himself, that his interest cooled somewhat, particularly when he found that they were doing a lot of research work in the Hebrides. I think it was very good research work, but it didn't sufficiently reflect his very wide interest geographically. He was



interested in the early civilizations and in Cambridge we have a very good department of classical archaeology, which Professor Anthony Snodgrass leads. Our specialists in Near Eastern archaeology, Professor [Nicholas] Postgate, and Dr. Joan Oates, who has now retired, met and conversed with Dr. McDonald a great deal, as did Barry Kemp, our Egyptologist. All these people were very helpful in meeting and encouraging Dr. McDonald. Indeed, they've subsequently had space in the institute and support for their projects. Dr. McDonald was on the awarding committee of his fieldwork fund during his lifetime and he approved the specific grants we were making. I think when he came here he found the wide geographical coverage he hoped to find, and probably it was a circumstance that my book *Archaeology and Language* was dealing on the rather wide geographical plan that interested him. (He had read the book in the summer of 1989 and it was that which first led him to contact me.)

SMITH: In 1986 you were elected Master of Jesus College, and you left St. John's to come here.

RENFREW: That was something which again was surprising to Jane and myself. We received a letter in 1985 saying they were in the process of electing a master, and would I be willing to let my name go forward. That came as a surprise. I don't know why such an eventuality hadn't occurred to me. In retrospect, it's clear that colleges have to appoint masters and so look either to persons outside the university or indeed



inside the university, so it's natural to scan people in various subjects. It was clear at the outset that the position was going to take time, and I think the post has indeed taken up a lot of time which otherwise would have been devoted to archaeology. On the other hand, I take the view that you only live once; it's interesting to do varied things, and it's proved to be very rewarding in a number of ways, and I hope successful from the College's point of view.

As for the distractions from archaeology, I do comfort myself with one or two thoughts in that direction. For instance, when Dr. McDonald first came to the department, it might have been difficult to entertain him quite so effectively if we hadn't been based here. If you are living in a college, then you are living in a master's lodge, so you have premises where you can entertain very well. If you want to give a dinner party, all you have to do is discuss it a few days in advance with the manciple who runs the college kitchens. The butlers will come in and set it up, order the menu, and they will produce a very good meal and clear it away again, so the whole thing can be done really very elegantly; it was possible to provide that sort of backup, as it were. Dr. McDonald wasn't a terribly relaxed conversationalist, so when he did on occasion dine with us on the high table, he didn't enjoy it as much as having a good conversation with a few people in a private dinner party, where you stick to the point and you're not suddenly breaking off to make conversation about something else with the person on the other side of you, or across the table from you. He wasn't a person



for minor niceties of politeness in that way, which are really part of high table life.

So, although being master has taken up quite a lot of my time, I rather doubt if we'd have got to where we did get to with the McDonald Institute if it hadn't been for the possibilities which Jesus College offered.

SMITH: What do you see as your primary responsibilities as master, both in terms of the maintenance of the college, but then also perhaps pedagogically. What are your goals?

RENFREW: Well, the statutes are unspecific: they say that you have a general superintendence over the affairs of the college. That doesn't necessarily mean a great deal. You certainly do chair all college committees, including the college council, which is the governing body of the college, but the college has permanent officers: senior bursar, senior tutor, domestic bursar, who are all fellows of the college, and then of course a large staff who are paid to do the things they do. Any master who thinks he's going to move in and the college will follow his inspired leadership in all respects is in for a rapid reorientation. Recently, there was a very well-known broadcaster in England, John Tusa, who was elected the president of Wolfson College, one of the newer colleges, with a strong emphasis on research. He came to Wolfson College, and then about six months later he left in great dissatisfaction. I think perhaps he saw himself as some sort of chief executive, which is not the way it works, really. I think that the Master also has a wider role in bringing people



together—those with different interests, and across the generations.

Jesus College I think was already in many ways well run and is still in many ways well run, but it had one or two problems. The historic buildings are enormously expensive to maintain, and it was necessary to have some sort of appeal to help pay for the very expensive refurbishment of the buildings. The college does have an income from endowment, but renovation costs were eating it up in a terrific way. So one task was to think in terms of an appeal, and we decided to link that to the quincentenary of the college, which is this year, 1996. We also had other objectives, one of which was to build new buildings, and in fact we have built one, which is the college's very beautiful new library.

[Tape VIII, Side Two]

RENFREW: In my role as master, I try and maintain the old college tradition, which is in some ways beginning to wither, of having interchange between the fellows, the faculty that is to say, and the students of the college. (I have tried to establish good contacts with junior members of the college in various areas, and have very much enjoyed doing so.) For instance, I decided early on, since the most famous sport at Jesus, and numerically the most significant, was rowing, that I would take an interest in the boat club. Also, rowing is equally popular with the men and women of the college, so in taking an interest in that sport one would be taking an interest in something which both men and women undertake. So I go and pedal up and down



the towpath when the Lent races or the May races are taking place and have found this and other comparable involvement with the students very rewarding.

I try to take an interest in student affairs in various ways, in the music of the college, for instance, and I also saw that there was real scope in a college like this for a strong interest in the visual arts. There was already a student picture loan scheme, which had been going for several decades, with very good collections, but it seemed natural to join with those other fellows who were wishing to take a stronger interest in the visual arts. So we have done that over the past decade, and it has been rewarding in a number of ways. It's been rewarding to do things for the college which are of interest to the students. Personally, it has been very enjoyable because it has given me the opportunity of getting to know a lot of artists who have become not only friends of the college but personal friends. As I see it, the master does have an important role. Very few fellows have really the time to give to take a strong interest in a range of student activities, and I think that is something that one can do.

SMITH: I have been struck these last few days by what would appear to be the large number of students you know by first name.

RENFREW: We do try and get to know the students well. I mentioned to you [off-tape] that our elder son was injured a couple of years ago, and this involved a certain investment of time, going into hospital to see him and so on, so over the past two years I've not had quite as much time as I'd have liked. I used to spend quite a lot of



time deliberately watching soccer matches, and taking an interest in student affairs. I still do with the rowing, and in that way one does get to know the students. I think it's quite clear that once one takes a lead in getting to know the students, then very soon you begin to dissolve the inhibition that they may have in talking to you, and so you can build up not necessarily a close relationship but one of civility and even friendship, whereby students give you a wave and you give them a wave. That is in a way quite significant, because I think it's part of the way they look at the college as a whole. So I think one can make a contribution to staff-student relations, which I think is really what the Cambridge colleges are about. I'm not sure that one can do a great deal for the intellectual life. One can support the musical life, one can do something in a field like the visual arts, one can encourage other societies which are focusing on other subject areas, but I'm not sure one can do a great deal more than that.

SMITH: Could you give me a sense of how a typical week in term might go?

RENFREW: Well, it's been of course further constrained by the fact that I go to the House of Lords for one day a week now, and all these things together really take too much time. In many ways it will be quite a satisfactory thing when, after one more year, my mastership ends, because I am aware that I am doing too many things at once. But before I was involved with the House of Lords, probably I would be going down to the department on Monday morning and lecturing there, and doing some



research. College committees generally meet on a Monday afternoon, and I would have dinner in hall on Monday evening. It would be a similar pattern on successive days. I would try and watch a little sport perhaps in the early part of summer afternoons. Naturally, I have to spend some time with my secretary in college dealing with routine college business, which is really quite extensive, as well as with my secretary in the McDonald Institute, regarding Institute business. I try and see some research students whom I might be supervising. I dine in college a couple of evenings a week, and hope to do some research in tranquility on a Saturday. On Sunday I probably see some student sport again, go into chapel on a Sunday evening at six, and so on.

(Then there is a good deal of entertaining: a sherry party for students on most Sunday mornings, usually a dinner in the Lodge on Saturday, perhaps for a visiting politician, often with student guests also, as well as fellows. Quite often we have lunchtime student concerts in the Lodge. And occasionally we have breakfasts for the boat clubs.)

During term, there is not a great deal of opportunity for traveling. There's nothing to prevent one's doing so, except one finds one's missed a great deal through being away, because during the term there are a lot of student activities; there are of course formal dinners and feasts, at the beginning of the academic year there are sherry parties to meet all the students, and towards the end of the academic year there



are sherry parties for the third-year students to say good-bye. Then of course there are college guests to entertain as well as academic guests. There is a whole range of college committees: in the department there is the faculty board, and the departmental committee. When I came here I was head of the department.

We had another piece of very good luck, which perhaps I should also have mentioned in terms of things happening in the department. There had been problems with the Pitt-Rivers Museum in Farnham in the 1970s, and there was a question as to whether part of it might pass into public ownership. General Pitt-Rivers, who had formed it, died decades ago, and his son and then grandson had owned it. The grandson had died, and it wasn't clear what the future was going to be. So the British collections did go into public ownership and went to the Salisbury museum. Quite out of the blue a friend of mine, Kenelm Digby-Jones, who was acting as the agent for the widow of Captain George Pitt-Rivers, rang me up and asked if it would be possible to found a named chair in Cambridge. So I said yes, it was very possible. The chair I actually held, I pointed out, was a named chair, founded by John Disney in 1851. It's a very good way of commemorating somebody. My friend asked how much that would cost, and I said I imagined it would cost in the order of a million pounds or so, but I would check up on that. So I consulted again the secretary general of faculties. This was before Dr. McDonald appeared on the scene, by the way. The thought was that there was wealth in the Pitt-Rivers family, and that it



might be a rather appropriate thing to commemorate Captain George Pitt-Rivers.

The university authorities thought it was a very good idea, and a sum was nominated that was required to endow a chair in perpetuity.

I initially thought it might be a very good idea to have a chair in Far Eastern and Pacific archaeology, which was an area that we very much needed to cover. On the other hand, it is an area of perhaps minority interest, so on reflection I thought, and others agreed, that a chair in archaeological science, which means of course the scientific techniques that are used in archaeology—hard science in that sense—would be a good thing. So I proposed we should have a George Pitt-Rivers chair in archaeological science; this found favor with the Pitt-Rivers family and various trustees who had to be involved, and this is indeed what happened. The George Pitt-Rivers chair in archaeological science was endowed, and then a panel of electors had to be set up, just as we were saying. In this case we did interview, because it was in some ways a rather new field, and Professor Martin Jones was appointed. He is of course an archaeological scientist, and he is a very energetic man in that field.

After he had been in position for a couple of years, it seemed appropriate to suggest that he might become head of department and undertake the departmental administration, or most of it. That was just as well because then I became the director of the McDonald Institute, which obviously involved other administrative responsibilities. The McDonald Institute naturally takes up some of my time, and the



matter has been complicated by my feeling the obligation to spend every Tuesday, in London, in Westminster. Sometimes, during term, particularly in those periods when I have a lot of lecturing to do, one really can be on the go from morning to night. I may spend an hour in the morning with my secretary. I usually have thirty pieces of mail in the tray—some of it junk but not all of it. Then I go down to the department for teaching or some lecturing, and maybe some committee work here in the afternoon.

Very often Jane and I round off the day around ten o'clock. If we're not doing anything else, we will pop across to the student bar for a drink, which is relaxing in itself, and also surprisingly refreshing, really. If there are no students that we particularly know to talk to we just have a drink together and chat with the barman. Very often there are students, not always those we particularly know, who come and talk about this or that, and they are always full of their own enterprises in an interesting way. So nearly always we come away thinking, "Oh, that was pleasant, that was interesting!"

SMITH: For those who aren't aware of the system, the college has a bar on campus.

RENFREW: Indeed so. There will be many who are not aware of the Cambridge system of having some twenty-five colleges, and the colleges of course are residential units, but they are much more than that: they have a chapel, a library, a hall, residential accommodations and sports grounds, which in this case are attached to the



college. There is only one other college which has all its sports fields around it. And one of the foci of social life is the college bar, which is open every evening from 6:00 P.M. till 11:30 P.M. and sometimes in the party room adjacent there are discos and all this sort of thing, so the bar is a good place to pop in. Sometimes there are those who drink too much alcohol, but there are plenty who just drink soft drinks. The very serious sportsmen, while they are approaching some great sporting climax are drinking only soft drinks. So it's a very good social center.

SMITH: Perhaps, since you've brought up the House of Lords, I should ask you, what was the process by which you were elevated to the House of Lords?

RENFREW: Well, I'm clearer about that now than I was. You receive a letter from the Prime Minister, and in my case, it made reference to the category of Working Peer. There are those appointed who are just persons of great eminence, and generally towards the end of their working lives they may receive such a letter saying that the Prime Minister is recommending to the Queen that they should be appointed, or elevated, as a Life Peer, and usually they agree, and then they take their seat in the House of Lords, without any obligation, although it's hoped they may take part in matters which they know about. But about ten years ago it was felt that there needed to be more activity in the House of Lords, and therefore persons of all parties would be appointed as Working Peers. The "working" not intending necessarily to imply that other peers didn't ever work—although that would be the case with some of



them—but rather that there would be an obligation to take part actively in the life of the House of Lords. So these Working Peers would be people who were still active in their own professions, not people who would necessarily be of retirement age. There is often some political element there. The Prime Minister of the day will appoint a number from his own party, but will also consult the leader of the opposition and of the Liberal Democrats for their nominations. I'm not quite sure by what process those without political affiliation are appointed. I am a Conservative, and that was clearly known. I did once years ago stand for election to Parliament, though I wasn't elected. It was a safe Labour seat that I stood for, so I didn't really expect to be elected.

Anyway, I received such a letter, asking if I would be willing to be elevated to the House of Lords on the understanding that I would be a Working Peer. I thought about this and I really didn't know what it meant in the sense of what the time commitment would be. I didn't know how the House of Lords worked. So I rang up the government chief whip, whom I knew slightly, and asked for guidance there. He said he would look into it, and the word came through that they thought it would be appropriate to do perhaps two days a week in the House of Lords. Well, I thought about this, and it was clear to me that there was no way that I could devote two days a week to being in London, when I was already at that time head of the archaeology department and professor of archaeology, master of the college, and also director of



the McDonald Institute. I had to say this really wasn't very feasible. So I wrote a letter to the Prime Minister, saying how very greatly honored I was, but to my very great regret I felt that it wasn't really possible to take on such a position. Then I had a very positive telephone call from the chief whip, and indeed from the secretary of state for education at the time, whom I also knew personally—Kenneth Clarke, he's now Chancellor of the Exchequer—and they both said, "Well, we've been talking about this more, we've thought it over, and there's a feeling that if you do one day a week that would be all right. Could you do that?" So I said yes, certainly that would be feasible, and then a formal letter came appointing me.

Then of course you have all the formalities of admission to the House of Lords. First of all you have to go and see Garter King of Arms, who is the senior herald. There is a college of heralds in London, which has great specialism, precisely in heraldry, and they deal in titles and also in coats of arms. Interestingly enough, I had already previously applied for a grant of arms just out of interest. I had come to know a herald quite well personally and he had suggested that I might wish to apply for a grant of arms. So that wasn't the issue, but you had to get your title straight. If you are Mr. Smith you can't necessarily assume you can be Lord Smith, mainly because there either is or has already been a Lord Smith, so if that's the case then you have to have some diacritical nomenclature; in other words, you have to be Lord Smith of somewhere or other. So you have to discuss with Garter King of Arms



where "somewhere or other" might be. So there is a title Baron Renfrew, which is one of the subsidiary titles of the Prince of Wales, as it happens. In fact, King Edward VII, when he was traveling very incognito, used to modestly call himself Baron Renfrew, which indeed he was entitled to do, since he held that title. So it had to be Baron Renfrew of somewhere, so after much discussion and thought I proposed Baron Renfrew of Kaimsthorpe. If you have a curiosity I could explain why, but it's not terribly relevant. Then you have the ceremony whereby you are introduced into the House, and you have to invite two peers of the same political persuasion to act as the persons introducing you. I knew Baroness Trumpington quite well, and because I had already been on the Heritage Commission, I knew Lord Montagu of Beaulieu, so I invited them.

On the appointed day we had to dress up in our robes and Garter King of Arms undertook a rehearsal. It is a very simple but rather strange ceremony. The Lord Chancellor is already on his woolsack, which is where he sits, and he puts on a three-cornered hat of the kind that was common in the eighteenth century. Then Black Rod, who is one of the officials, precedes the procession, carrying his wand of office, followed by Garter King of Arms, dressed in his wonderful embroidered clothing bearing the Royal arms, and then Baroness Trumpington appears, carrying her own cocked hat and dressed in the red robes of a Baroness, then myself, in the red robes of a Baron, and then Lord Montagu, likewise. So we march in, in procession,

[The text on this page is extremely faint and illegible. It appears to be a list or index of items, possibly names of people or places, arranged in several columns. Some faint words like "List", "Name", and "Address" might be discernible at the top, suggesting a header for a directory or record book.]

bowing at several points to the Lord Chancellor.

The new Peer has to go up to the Lord Chancellor, and the Reading Clerk hands him the scroll of appointment, which then the Peer has to hand to the Lord Chancellor, who hands it back to the Reading Clerk, and then you process back to the Reading Desk, and there the Reading Clerk reads out from the scroll: "We, Elizabeth . . . by the grace of God . . . sovereign . . . " and so on—it's a long text. Then there's another text to be read, and then you take the oath, that you will "bear true allegiance," and so on. And then the procession moves to a back bench, and there all three sit down, put on their hats (except for the Baroness), stand up, raise their hats and bow to the Lord Chancellor, who takes off his hat and bows in return. This happens three times, and then the procession moves down again towards the Lord Chancellor, and the new Peer shakes the hand of the Lord Chancellor, and that is the end of the ceremonial. All the Peers together say, "Hear, hear," to signify their general approval of the matter, and then you leave the chamber and get rid of your red robes. Subsequently, one of the Peers comes in with you, and you sit down with him on one of the benches. You have "taken your seat." After that you are entitled to take part in debates, although, before you can speak you have to go through the procedures of your maiden speech, which is another story . . . perhaps it will burden to have too many anecdotes about the House of Lords. But after a while then you become a functioning Peer and you can indeed speak in debates and vote.



SMITH: From what you've been mentioning over the past few days, it seems that you do in fact put a fair amount of time into this responsibility. In addition to being in the House of Lords, you sit on committees.

RENFREW: That's right. Fortunately it has worked out that most of this can normally be compressed into a day a week, although because you're never quite clear when the House will rise if you stay to the end of proceedings on that day, I usually stay in London overnight. It turns out that although you can't always predict the passage of business, it's best from my point of view to go in on a particular day of the week, and so I go in on Tuesdays, when there are often quite important debates and issues to be discussed. I was also asked to become a member of the select committee on the European communities. The House of Lords has a good system of select committees which conduct special studies and then publish reports, which are really very highly regarded, and the committee on the European communities is well regarded in Brussels; it gives very detailed reports and analyses of questions. I was also appointed to the subcommittee on financial and external affairs, which is very interesting. The report we are working on just now is on the question of European monetary union, which is a very hot question politically in Britain at this time, and that has proved to be absolutely fascinating. It was not a matter I knew a great deal about initially, but we've heard a great deal of evidence, and the report will be informative and will show the very wide range of opinion among very well informed



specialists: financiers, industrialists, politicians, and so on.

Rather to my surprise, I was asked if I would chair the library and computing subcommittee, and this has more to do with the administration of the House of Lords. The House of Lords has a library which has been of good reputation for a long time, and recently it has been taking computing seriously. All the Peers' rooms and offices are being cabled up on a network, and the Peers who wish can apply to have a word processor at their desk, those who have desks, or otherwise borrow a portable word processor. So the system is being kitted out in quite a sensible way. I don't think it would surprise many of the more advanced chambers of government in the world. I'm sure the backup in the Senate or the House of Representatives is much more ample, but nonetheless, it's something that is worthwhile. I found to my astonishment that because I'm on that committee, I'm on most of the other administrative committees, in case there's any library or computing business on those. Fortunately, they all meet on a Tuesday as well, so it means my Tuesdays are well filled. But I don't actually object to that, because I think it's easy in the House of Lords to go and not say very much and not do very much, and really wonder what you're doing there, whereas on these committees one is taking part in the life of the House, and also seeing how it works. Like all these things, before you can really operate effectively within an institution, you need to know a lot of the people and understand how it works. So although I'm only there one day a week, I think I would be regarded as one of the active members



of the House, which is indeed what Working Peers are supposed to be.

SMITH: Had you been knighted?

RENFREW: No, no. Some heads of houses are and some people are knighted for academic reasons, like Sir John Boardman, whom you know, or in Cambridge, Sir John Lyons was knighted for services to linguistics. Grahame Clark was knighted a few years before his death for services to archaeology. So that can happen, but it certainly hadn't happened to me.

SMITH: Had you known Margaret Thatcher well prior to your appointment to the House of Lords?

RENFREW: Not well. The Prime Minister at the time was John Major, but I hadn't known him well either. I had met Margaret Thatcher a few times. I'd only been to 10 Downing Street on one occasion, which was when she gave a reception for the commissioners of English Heritage at its inception, in 1984 I think it was. I also met John Major a few times. I don't know him well now, though I respect him very much.

SMITH: And you are a member of the Conservative party. Were you very active in its activities?

RENFREW: Not hugely so; I joined the Bow Group when I left university. The Bow Group is a group of people who are perhaps on the more left wing or progressive side of the Conservative party. At the same time I also joined a dining club called the Coningsby Club, where those who are interested politically go to



dinners held in the House of Commons, or sometimes held in a London club where a speech would be given by a cabinet minister or somebody of that rank. I've kept in touch with the political world, because when I was in the Cambridge Union Society I came to know a lot of contemporaries who were also active. It has been a strange phenomenon, commented on quite widely in the press, that a great number of people of that generation have become cabinet ministers. For instance, in the present cabinet, Michael Howard is Home Secretary, John Gummer is Secretary of State for the Environment, and Kenneth Clarke is Chancellor of the Exchequer. Norman Lamont, Kenneth Clarke's predecessor as Chancellor of the Exchequer was also broadly of the same generation. The vice president of the European Community, Sir Leon Brittan, was a close friend from the same time. So I knew a lot of people in the political world.

As I mentioned, in 1968 there was a byelection in the constituency of Sheffield Brightside resulting from the death of the member, and I had put my name in to the local constituency association and been nominated as their candidate. Indeed, it was just a three weeks byelection campaign, but it was a very interesting experience because of big press coverage. It was a time when there were large swings away from the Labour government to the Conservative party. The Labour majority had been nineteen thousand, and we cut it to five thousand, so it was a very significant swing. After that I was invited to become chairman of that constituency association.



That ended of course when I left Sheffield, and I wasn't so active in Southampton.

SMITH: Were you generally supportive of the reforms that the Thatcher government instituted?

RENFREW: Most of the earlier reforms, yes, though there were mistakes of course: the poll tax business was a terrible mistake, but I had never looked very deeply into that. Later, when I got into the House of Lords, the council tax legislation went through, which was much better, and so I supported that actively. Like many in the Conservative Party, I felt that Mrs. Thatcher became, in her later years, rather autocratic in manner, though I didn't have first-hand experience of that. She also became increasingly hostile to Britain's continuing effective participation in the European community, and I was very shocked by that. I didn't think she was taking the right line. After she dismissed Geoffrey Howe as Foreign Secretary, he rather unexpectedly—because he always seemed a rather mild-mannered man—made a very decisive speech criticizing her in the House of Commons, and I absolutely agreed with him. I thought it was the appropriate thing when the Conservative Party decided to choose a new leader, and though I found John Major a slightly unexpected choice, because he wasn't a particularly prominent figure, he turned out in many ways to be an excellent choice. I think one could make some criticisms of what's happened in government in recent years, but I do still think of Major as an excellent Prime Minister, and also really a very pleasant and sensible man.



SMITH: It seems like you do a fair amount of fund-raising in your work.

RENFREW: It's fallen to my lot, yes, although the most successful occasions have been undoubtedly those where the donor has come to us; this was the case not only with the Pitt-Rivers endowment and the McDonald endowment, but in the college also. One of our most substantial donors has been a lady who did not have links with the college, but she was put in touch with us and she came to like the college very much. I'm sure that's one of the lessons of fund-raising, but you obviously have to create in some ways the circumstances where people are inclined to get in touch with you.

SMITH: Do you enjoy fund-raising?

RENFREW: I very much dislike asking for money, but the cases I have been speaking of are cases where the donor was already in touch, so one has to continue to create an ambience where they like it and enjoy it, and yet one can't sit down and say, "Excuse me, when are you going to sign the check for half a million pounds?" or something. I actually find it difficult to ask for money directly. I don't think I have a great gift for that, but on the other hand, to set up a situation where it seems natural to give money can be very agreeable, to create a pleasant environment.

SMITH: Other people I've talked to in British universities have felt that the last fifteen years has been a period of both crisis and opportunity in the sense of government funding.



RENFREW: More crisis than opportunity. Where is the opportunity to which you refer?

SMITH: Well, if one has an entrepreneurial spirit—

RENFREW: Right. Yes, that may be so. On the other hand, if you are talking about a department where you are seeking to find ways of deriving money from the system, you have to work at it very hard, and you have to read the parameters very carefully and judge which way the government is going to go. If you're not careful you are spending most of your time doing rather bureaucratic things so that the statistics come up right. I think that is hard work.

SMITH: How have you felt about the changes that have occurred, as an academic on the one hand, but also as a member of the Conservative Party?

RENFREW: Dismayed. In fact, for most of my academic career, really, I've been saddened at the way governments have consistently put priorities in higher education on the back burner, mainly because many governments tend to respond to pressure.

Harold Wilson's government was a very good example of that. I never thought highly of Harold Wilson. He seemed to be a man with no principles or policy other than to stay in power. I don't mean he was an evil man, far from it, but he just didn't use his opportunities to do any great good. During his time those groups that were able to mount an effective strike got recognition; academics are not in that position, so that was a period when academic salaries fell far behind others in real terms, and that



remains the case.

I think governmental actions have mainly been deleterious to the universities. For example, just after I entered the House of Lords, the government had a Higher Education Act, one aspect of which was to broaden the university sector by declaring those institutions of higher education, polytechnics, and so on, to be universities, and thereby at a stroke enlarging the university population by a very large measure. I thought that was in the main a good idea, because there has been a lot of snobbery in Britain about the different characters of academic institutions, and some polytechnics were quite worthy of respect. But at the time, I and others pointed out that if you are going to do this you may have to put in further resources. For instance, the existing universities all had significant research components, the existing polytechnics did not. So if you were going to turn the polytechnic into a university overnight, you'd probably have to offer its staff members some opportunity for undertaking research.

All this was blindingly obvious, but now, four or five years later, the government suddenly realizes with cries of alarm that it can't afford to fund research in the more recently created universities on the same scale as it did in the preexisting universities. Previously, the universities were generally well respected internationally, and the polytechnics were probably well respected too, but now that they've all been declared to be universities, overseas governments are making up rank tables to determine which are the best for students to go to and which ones they'll fund and not



fund scholarships. Of course it's not easy for a government in a difficult economic situation to pour money into every sector, but I think they've succumbed to quite understandable public pressure and put much more money into health, which is perhaps no bad thing, and the higher education sector has been very seriously underfunded. This applies to all kinds of research, including scientific research, and that will probably prove to be a mistake in the long term, I suspect. It doesn't follow that you have to have your innovations produced from within your own country, but if you have a great tradition for science and a good standard, to let it go by default by not having adequate research programs is very sad.

SMITH: I wanted to shift a little bit to your students, particularly your research students. Over the past thirty years, I wonder if the questions they bring into a discussion with you and the subjects that they are inclined to research have changed in any considerable way?

RENFREW: I think the aspirations have indeed changed. No doubt students thirty years ago were setting out with what we would consider traditional questions, concerning basic issues of chronology and culture contact, and now they come with much wider questions. The truth is, if one is discussing with students subjects suitable for research, and specifically for doctoral dissertations that will be worthwhile and merit the award of the degree, it's often very difficult to encourage them to focus on the big questions that need grappling with, because many of those questions



probably need a project of more than three years duration to have much impact. So I quite often find myself slightly frustrated in advising students to choose maybe a rather limited body of material, which they are going to know more about than somebody else. Sometimes, if they come up with very broad questions, I point out that when they've written the dissertation they won't know more about any specific topic comprising their field of study than others, including the external examiner. I often feel it's appropriate that a graduate student with a good theme will actually end up knowing more about the specific matters they have studied probably than anybody else alive, so it often is quite wise, I think, unless they come with a very well prepared theoretical framework in which to operate, to advise them they'd do well to choose a restricted body of material about which they will be particularly well informed, and if its unpublished material, obviously, more well informed than anybody else.

I don't think that's a terribly challenging position intellectually, but on the other hand, if one is supervising a doctoral student, one is hoping, as indeed are they, that they are going to end up, after not much more than three years, with a doctoral degree. In a way I think that's the first priority. Also, I sometimes have to point out to students that what they are writing is a Ph.D., which is an exercise, a research experience over three years; it's not the definitive textbook on an entire field. Students very often tend, if they're not corrected, to write rather poor dissertations because they are tackling too much, and in consequence, unless they are very well



focused on some crucial issues, they are probably not saying very much.

SMITH: What have been the more interesting dissertations that you've directed?

RENFREW: Well, I've had a good range of dissertations that have broken new ground. When I was in Sheffield I had a very interesting graduate student who was writing on prehistoric textiles in Britain. Not primarily from preserved textile finds but from ancillary finds, iron age weaving combs and mat impressions, or impressions on pottery, and that told us a lot more than we knew before about textiles. I had one student who's now at the University of Michigan, John [M.] O'Shea, who's well established now in the Department of Archaeology. He was very interested in looking at the archaeological potential of burial customs, and he had a great deal of material relating to one specific North American Indian group, where one did have some very clear insights from anthropologists into the social structure of the group as well as archaeological and other evidence about the nature of the graves, so that was all very interesting indeed. I've had two students, a husband and wife team, writing on the prehistory of Italy and Sicily, which was very good systematic work. And many more besides. So I've had a good range of students, but, as I say, I think the students are quite rare who during their doctoral dissertation period produce ideas of massive originality.

[Tape IX, Side One]

SMITH: Do students come here because they want to work with Colin Renfrew, or



do they come because it's Cambridge?

RENFREW: Both. We have a great many applicants because it's Cambridge, but also quite a few graduate students write in and say they want to work with me. Most of them are not necessarily well informed in that respect. I may be somebody they have heard of through their reading, but they may not be particularly well acquainted with my particular interests, nor am I necessarily well informed to supervise them. I certainly always find the supervision of research students quite difficult, because one always has the nagging feeling that one's got to really ensure that they are helped to come up with the basic requirements of a Ph.D. It's much more interesting to talk about the most interesting ideas they have, but those don't necessarily constitute an entire Ph.D.

SMITH: Do you spend much time with undergraduates?

RENFREW: Yes, I do quite a lot of undergraduate teaching still. At the moment I'm doing quite a lot of first year teaching. I always enjoy lecturing because it does make you straighten your ideas out and try and put them across. I find that quite refreshing. Before I was a professor, I used to do a great deal of undergraduate supervision, in the sense of setting essays, grading them, and discussing the work. I don't do much of that now because I simply don't have time. But it does mean I'm not as closely in touch with the archaeology undergraduates as I used to be. It's my intention, when I finish here as master, to spend rather more time in the department.



SMITH: I was wondering if you often set problems for your research students. I mean, you may have a set of concerns that are occupying you at a given moment, and you can only handle certain aspects of these, and there are other aspects that would be interesting to find out about.

RENFREW: That could be the case, but most graduate students come with some clear ideas of what they want to do, and then one sometimes has to point out the shortcomings of that approach, how it could be modified, but not usually radically changed. It's unusual to have a student coming up and saying, "Here I am. I haven't the faintest idea what I should do for research, what do you suggest?" I remember when I consulted Glyn Daniel about what he thought might be a suitable research topic for my research, and he came up with the east Mediterranean origins of the chamber tombs of the west Mediterranean. I could already see this wasn't the ideal topic for me, so he was very positive when I said I wanted to study the Cyclades, and he encouraged me in that. I think it's probably quite a big responsibility to assign Ph.D. projects to students. I think they should look into them for themselves to some extent.

SMITH: Another major aspect of your activity has been with various boards and commissions concerning archaeology and national heritage in Britain, and you're also a member of the European Community. I understand you're a member of two committees concerning European archaeology and the Human Genome Diversity



Project?

RENFREW: Yes, some of those have more reality than others, it must be said. For many years, since the early seventies, I have been a member of the Ancient Monuments Board, or its later equivalent, which is the specialist body that now advises English Heritage on archaeological matters. It does have a responsibility for trying to set national objectives for conservation, and because salvage archaeology is largely funded by English Heritage, it also has a supervisory role over much of the practical excavation work that goes on in this country. So that is interesting and positive. Then I was a member of the Heritage Commission itself, which is the governing body of the enterprise, for many years, but I gave that up when I became master here. I think one does have an input in these matters, though it can be a bit bureaucratic, but there are other committees that have no great meaning. I'm a member of the standing committee of the Union Internationale des Sciences Pré-et-Proto-Historiques, the UISPP, and that has a conference once every four or five years. I tend to go along to the conference, but it's in some ways rather a traditional body and not a very exciting thing to be involved with.

The Human Genome Diversity Project is related to the Human Genome Project, and it does have interesting underlying issues. The Human Genome Project, as I'm sure you know, has the intention of establishing the molecular structure of the genes of humankind, so it's a huge body of information, but it's gradually being



worked on. Framing it in that way somehow suggests that all humans have roughly the same genes, and it isn't in doubt that all humans have a sufficient degree of similarity in their genetic composition to distinguish them from other apes, but there is much diversity which is well worth studying. So the Human Genome Diversity Project has taken the opposite line: not that we can think of a standard human gene, but what is the variation within the genetic composition? And that leads immediately on to the questions we were discussing, about the genetic understanding of human diversity.

SMITH: In the United States, as you are probably aware, this is a major controversy—the way the genome project has been set up and the ways in which the federal government should be supporting it. As a member of the House of Lords, are you in a position to affect how the British government decides what it's going to do vis-à-vis its participation in the Human Genome Project?

RENFREW: Only very indirectly, but I don't think the British government has any participation in the Human Genome Project in itself. The Department for Education, through the research councils, undoubtedly does fund a lot of very good biochemical work, and some of that may be interacting with the Human Genome Project. But I have seen some of the controversies; for instance, it seems to me very doubtful wisdom that companies can patent whole stretches of the human genome which they have elucidated. Although there are in fact some reasons for doing so that have been



explained to me, it seems like patenting parts of nature. If you find a new species, you don't patent it. Who are you to patent a preexisting species? That is what I feel about DNA sequences: who is some researcher to patent that sequence? Of course, arguments are made that before you can use its information it requires a great input of investment, and you must protect your investment and so on. I think Jim Watson, who was in some very central position within the project, actually resigned when the United States took the view that you could legitimately patent lengths of DNA coding which you had elucidated. I entirely agree with him; I think it was an extraordinary decision.

So there are many controversies. The Human Genome Diversity Project runs into different, but related controversies. If one is taking samples from what to us are obscure tribes in Africa or in Asia, and you get a tribe saying, "Look, you're exploiting us, and we insist that you stop taking such samples," to some extent their complaint may be warranted. There have been cases of very rare genetic variations being worked up and studied up after being collected in Africa and possibly made into drugs with a high sale value. So the concept of informed consent, at one time something to which you would just pay lip service, is much more vigorously debated now. What is well-informed consent of a subject who has agreed to give blood? What do you do when in your blood bank you have perhaps very interesting data from somebody who died twenty years ago, who gave consent for you to use their



blood samples, presumably by implication, during their lifetime? Unfortunately, the Human Genome Diversity Project has become slightly bogged down, as to some extent has the Human Genome Project, in some of these essentially political issues.

SMITH: But you being in the Lords have a political role.

RENFREW: Yes, but that doesn't mean that one's observations on a whole range of matters are necessarily treasured and studied. I could formulate and set down a parliamentary question about the British government's position vis-à-vis the Human Genome Project and the Human Genome Diversity Project, and then a minister would have to answer, and then I could pose my searching supplementary question, as it were. If there's legislation before the Lords, then you can really become involved and make your comments and propose amendments, but if there isn't, how do you effectively bring it forward?

SMITH: Okay. In your various advisory and directing capacities, have your theoretical insights affected your participation or your evaluation of the projects that are being submitted?

RENFREW: Yes. In the Ancient Monuments Board, about ten years ago, when salvage archaeology in this country was going on at a very effective rate, questions arose as to how one should evaluate such projects. I was one of those who said one really had to have a coherent project design stating clearly the objectives, even though some of these were projects which had been supported for many years and officials



within English Heritage or within the Department of the Environment knew the work well. Gradually, my view came to be very widely accepted; I'm sure many other people had the same idea, but there's no doubt that one can have an impact by consistent input.

To give another example, I am a trustee of the British Museum. Already before I became involved there the trustees had decided, partly led by the then director, Sir David Wilson, that the museum should not be buying unprovenanced antiquities, but if you are on the board of trustees you can really press the point. You can ask, "Where did this from? and, "Should we be buying it?" Perhaps nobody else is worrying about that issue, but one can have an impact. The British Museum has been very good on these issues of late, it seems to me. As I'm sure you know, you really have to go to most of the committee meetings if you are going to cut much ice with the committee, so it's very time-consuming.

SMITH: Yes. While we're at it, in your handbook [*Archaeology: Theories, Methods, and Practice*], you discuss the claim by various nations, such as Turkey and Greece, that archaeological materials were pilfered from them and should be returned. What position have you taken as a trustee of the British Museum on these kinds of claims?

RENFREW: I haven't as a trustee been asked to adjudicate on restitution claims. I realize there's much more to say about that, but I've always taken the view that the



real problem is not who owns which objects now, although that may well be worthy of debate, but how do we stop the continuing looting? I feel very strongly that museums should not buy unprovenanced artifacts of recent origin—say, that have come on the market since 1970. I think if all museums agreed about that and agreed there would be no tax concessions for gifts of such materials, then the market would suffer a setback, which would be very good for it.

The issue of where objects should best be curated, objects that have been sitting in a museum not only for decades but for centuries, whose find spot is generally well known and has never been in doubt, has always seemed to me an entirely separate and secondary question. That being said, it's still a question that can be asked, and you have to judge each case on its merits. But that's my personal view. As a trustee of the British Museum, I would feel it my duty to be a little conservative in outlook and where possible to support the opinion of the majority of trustees unless I felt strong reason to seek to change it. If we are talking, for instance, about the Elgin Marbles, which I suspect may have just flashed through your mind as you asked that question, I think it is a difficult issue, and I think unless a trustee of the British Museum can present a totally clear rationale as to why the Elgin Marbles should be returned to Greece, I think it's fair to go very cautiously.

If I weren't a trustee of the British Museum I might be more enthusiastically inclined towards restitution, since I do understand and sympathize with the feeling of



most Greeks, that the Marbles have a special meaning for them. Since I am a trustee, I don't think we should rush to hand these materials back. I think the then Greek minister of culture, Melina Mercouri, made a public song and dance about the matter, but she actually undermined the Greek government's position. She was sufficiently extreme that Greek archaeologists instead of calling the Elgin Marbles, *ta Elginia*, the "things of Elgin," they started referring to them as *ta Melinia*, the "things of Melina Mercouri." [laughter] She really went so much to town on that that it became a rather cheap, nationalistic political campaign. That doesn't mean that ultimately perhaps they shouldn't be returned, but some of the appeals for their return fall within the framework of Greek chauvinism, which finds its most extreme expression, for instance, in the Greek attitude to the former Yugoslav republic of Macedonia. There still are arguments for returning them, but I don't think the best arguments are Mercouri's arguments.

SMITH: If by some fluke of circumstance the Lydian treasures had wound up at the British Museum instead of the Metropolitan Museum, would you as a trustee have supported their being returned to Turkey?

RENFREW: Oh, absolutely. We're talking about material which was recently acquired—more than ten years ago now—and it is perfectly clear that persons in the Metropolitan, like your friend Dr. von Bothmer, knew exactly what they were doing. It would appear that they knew they were buying looted material, and in my view no



respectable museum could have done so at the time of the purchase, and still less so now. So there's no doubt at all the British Museum would have handed it back, or rather that it would not in the recent past have purchased such obviously looted material anyway.

We had a very discreditable case recently, where the British Museum bought a series of very interesting bronze shields of the iron age from Lord [Alistair] McAlpine. He had been treasurer of the Conservative party, but he was also an antiquities dealer. This was a few years ago now; I think had the Museum been applying its more recent codes, it wouldn't have bought them. On the other hand, when you are talking about British antiquities, the British Museum does have to be an acquirer of last resort. If the objects are clearly looted but of British origin, and one asks what should happen to this stuff, it's sometimes felt that it probably ought to go to the British Museum, whereas if it is material of overseas origin, the right answer is to absolutely not touch it.

There are problems associated with being the repository of last resort for stolen and looted material from one's own country. It turned out these bronze shields were part of a metal detecting scandal, so the trustees felt obliged to return the materials to the legitimate owner, and they requested from the vendor return of the money expended, which has not been forthcoming. But the point of the story is that the British Museum unhesitatingly returned the material to the person from whose



land it had been plundered. It's rather rare to be able to document such circumstances, but it was documented with a chain of information which was quite plausible. So, yes, I would condemn the Metropolitan Museum unreservedly in the matter of the Lydian Treasure, and more than that, I think it is actually a scandalous institution in the way it acquires unprovenanced antiquities. Some years ago they bought the Euphronius vase, again under the guidance of the good Dr. von Bothmer. I'm very skeptical about his approach to museum acquisition. The museum acquired this Lydian treasure against all ethics. Of course, perhaps one mustn't totally criticize Dr. von Bothmer when the museum director, Thomas Hoving, was as bad. Nobody would doubt von Bothmer's genuine respect for the material. One might say that he doesn't have much sense of the context of where objects originally came from, but nobody would doubt that he has a real appreciation of the material and a deep respect for it, so he's in some ways an excellent and admirable scholar, whereas Thomas Hoving was just a buccaneer and perhaps even a showman. Have you read his autobiography [*Making the Mummies Dance*]?

SMITH: I've looked at it, yes.

RENFREW: Yes, well, it's inconceivable that a respectable institution would have a person with such an outlook as a senior official. I do not regard the Metropolitan Museum as a respectable institution. They mounted an exhibition of the Leon Levy and Shelby White collection. I blame the museum more than they: they were private



collectors, buying up all these unprovenanced antiquities, and they may have known no better originally, but a major institution like the Metropolitan should know better than to put on a public exhibition of recently acquired antiquities of unknown provenance. So I think it is a public scandal. And the Lydian treasure shows them up perhaps at their worst, certainly in a scandalous way.

SMITH: I think in the handbook you mention the situation at Sotheby's and their policies, which you deplore. I wonder if you've done any work to try to convince them to change their policies? Obviously, by citing them publicly you are trying to bring some kind of public wrath upon them.

RENFREW: I think one would have to be very idealistic indeed to imagine one could effect change by pointing out to Sotheby's their infringement of international decorum, or indeed infringement of the codes of practice to which they themselves have subscribed. I think one has to be more robust than that. (Since the interview, Sotheby's has responded to public pressure and has brought to an end their antiquities sales in London.)

SMITH: What about efforts to change legislation in this country?

RENFREW: Well, those efforts are being made. At the moment the Treasure Bill is going through parliament and will probably be passed, and that tidies up, to some degree, the rather curious situation concerning antiquities found in Britain. There's also a consultative document from the Department of National Heritage on portable



antiquities, which covers other categories of so-called treasure, and that may well lead to a voluntary reporting system, which is better than none. Most archaeologists agree that we should go for a voluntary system, though we might aspire to a mandatory system, because probably one wouldn't get a mandatory one through parliament or even introduced, whereas if you proclaim a voluntary system, maybe many will subscribe. If not, you can say, five years later, "It's clearly not working, we'd better have a mandatory system."

So that's respectable so far as it goes for antiquities of British provenance, but it is very scandalous that it is in no way illegal to offer for public sale antiquities that have been looted and illegally exported from their country of origin, as Sotheby's was doing, for instance, with the Sevso treasure. And that is because Britain has not signed the relevant conventions: the UNESCO convention of 1970 or the UNIDROIT convention. There the story becomes complicated because there are aspects of both of those conventions which would make good compliance difficult within our system, and the British government always cares to pride itself on very good compliance when it signs. Of course, what the British government should be doing is vigorously advocating some form of convention which it feels it could sign, which would offer genuine protection.

At the moment it's just flagrant the way works from overseas are sold in this country, certainly in complete breach of the code of practice of the antiquities dealers.



In fact, I'm about to put down a question for written answer in the House of Lords on that subject. I asked a question about the UNIDROIT convention and received rather predictable responses, but the minister made some reference to the voluntary code of practice which is followed by the antiquities dealers. Since it was referred to explicitly, I thought I would ask, first of all, would the minister kindly place a copy of the code in the library, which is standard procedure for making it publicly known; secondly, would she care to indicate how often it was followed and how often it was breached, in the views of the government; and thirdly, would she explain the systems the government has in place for monitoring the matter—which of course are absolutely none at all. I think she'd find it difficult to evade that point.

The situation is disgraceful at the moment, but it has been allowed because the antiquities market is an important one, and they are very effective in lobbying the government. In my view it comes very close to corruption in high places. The former chairman of Sotheby's is Lord [Alexander Patrick] Gowrie, who before he became chairman of Sotheby's was Minister for the Arts in the Conservative government. He's now chairman of the Arts Council, and given the things that Sotheby's did during his chairmanship, I think one could ask questions. I won't put it more firmly than that, because I don't wish to embargo too much of our discussions. I think if I put it that way then the lawyers can probably cope with that.

SMITH: Yes, we don't want to get involved in libel suits either. [laughter] And your



law is considerably tougher than American law.

RENFREW: Yes, there's money to be made if you play your cards right, that's true.

SMITH: Did you take any public action at the time that the Royal Academy exhibited the George Ortiz collection?

RENFREW: Yes I did. I was one of the people who drew attention to that. There's a television program called the *Late Show*, which is a sort of chat show kind of program, about ten o'clock at night, with a very wide spectrum of viewers, and they invited me to take part in a discussion on the merits of the Royal Academy displaying this material. Anna Summers Cox, the editor of the *Art Newspaper*, was there, Mr. Ortiz himself, and a very well-spoken man whose name I forget, who was connected with the Royal Academy, and to him everything the Royal Academy did was exactly right. So we had quite a lively discussion, and I certainly expressed my view that Mr. Ortiz was doing great damage to our shared heritage at the international level by buying this unprovenanced material. The fact that so much of the catalog was unprovenanced showed that it was far from being above reproach, and I implied that the Royal Academy had no business showing such stuff.

Later, I was asked to write up my opinions on the matter for the *Guardian*, The Academy at that time protested that it was doing a great service to humankind by showing these beautiful artworks, which was the usual argument: If Mr. Ortiz hadn't bought these objects, somebody else would have bought them, and it's so much better



that they are in the hands of so refined and sensitive a man. I managed to get on reasonably politely with Mr. Ortiz, so he and Piers Gray, I think it was, the secretary of the Royal Academy, invited me to see the show when it opened; I'd seen only the catalog. So I went, and who should I find there but, first of all, Jonathan Rosenthal, who is the man that initially commissioned the exhibition for the Royal Academy, and also the president of the Royal Academy, Sir Roger de Grey, whom I didn't know well. I felt it was incumbent on me, since I'd just criticized the Academy on television, to explain to the president why I had done so, and we had a civilized altercation. So the Royal Academy was left in no doubt, by me and by others, that its position was considered questionable.

The Royal Academy has a nominated trustee to the British Museum, a very brilliant sculptor, Allen Jones, and I took him to task and asked why on earth was the Royal Academy showing this stuff? He was on the exhibitions committee, which had never been consulted before the exhibition was signed up, so I think already there was some disquiet. Earlier this year, there was the *Art of Africa* exhibition in the Royal Academy. They wanted to borrow a considerable quantity of material from the British Museum, which of course has very important African ethnographic collections. The curators formulated the view that it would be inappropriate for the British Museum to lend material if the exhibition was also going to contain looted material. Well, this is a new principle, the notion of contamination, as it were. The



British Museum by then would not have shown the *Art of Africa* exhibition if it contained looted material, and it would never have shown the Ortiz collection.

So the Royal Academy was therefore left with a kind of ultimatum: do we do without the British Museum material, which was going to be very important, or do we do without the loans from private collectors of looted material? They made what was perhaps a tactical error and said they would consult the national museums of sub-Saharan Africa. Well, if you consult them formally they have to say the right thing, so they of course said we don't want all this looted material shown, so the Royal Academy had to exclude the unprovenanced material, with some exceptions, but nonetheless, the policy decision was taken. I regard that as a great triumph. Not only was the British Museum helping to make more general the standards which had already been well formulated, but also it began to clarify the future standards for two spheres of exchange: the objects which are of known provenance or were legally exported and so can be freely exhibited or indeed freely sold, and those objects for which there is no provenance, which no respectable museum would dream of purchasing, displaying, or even accepting as a gift or a bequest. The British Museum would never accept such stuff as a bequest.

SMITH: But then what happens to such material?

RENFREW: It goes to the Metropolitan Museum of Art. [laughter]

SMITH: At some stage you reach a point of saying it needs to be returned to its



country of origin.

RENFREW: You are quite right; you do come to that point, and maybe that is indeed the right answer.

SMITH: It was pointed out to me that much of the material in the Goulandris collection is considered to be looted.

RENFREW: That's right. I think much of it is the product of illicit excavations. The objects were bought by Mrs. Dolly Goulandris in the sixties and seventies. There are some mitigating circumstances: the collection was bought with the knowledge of the Greek government and the Greek antiquities service. They took the view, which was partly accurate, that by her purchasing it they were avoiding its being exported out of Greece, and for them that was the paramount factor. It's my understanding, though I've not actually seen it fully documented in writing, that that material will ultimately become the property of the Greek state. (I have no doubt as to the sincere motivation of Mrs. Goulandris: she felt she was doing the nation a service by preventing the material going overseas. And she has displayed it beautifully in a first-class museum.) Although that deals with the ownership aspects, it in no ways deals with the terrific loss to our understanding of Cycladic cultures resulting from those illicit and unrecorded excavations. It would be much better if that material had not come out of the ground, or even better if it had come out of the ground in controlled ways.

This is one of those cases where probably in international law, so long as the



material remains in Greece there is no legal objection to be lodged, but ultimately it can be legitimately questioned whether the Greek government is taking the right view. Many national governments are more concerned with ownership than with protecting information about context, so when the situation is considered from that point of view, I think there's no doubt that the Greek government should have acted much earlier to find out where some of this material was coming from, and should have prevented the looting that was continuing. I think, on reflection, it is true that the Goulandris collection undoubtedly falls within the context of unprovenanced material. It was of course shown widely internationally, and it was shown at the British Museum some years ago, but I think if it were coming up for proposal as an exhibition today, the British Museum might well say, "We really don't think we can show this unprovenanced material," and I think it might be right.

SMITH: Did you have any qualms about working so closely with them on *The Cycladic Spirit*?

RENFREW: Yes, I did. I thought quite carefully about it, and indeed said so in the preface to the book. I formed the view that it was material of such interest that it was worth writing about. I think in retrospect that may not have been the right decision; I think in all these issues moralities change and become clearer, but I doubt if today I would leap forward and publish a volume which was principally directed to the Goulandris collection precisely because so much of it lacks provenance.



SMITH: But doesn't a scholar also have a responsibility to document what exists, and interpret it, even when what has come forward perhaps should have come forward under other circumstances?

RENFREW: Well, this is what somebody like, say, Michael Coe, or indeed Dietrich von Bothmer would argue. I don't know Michael Coe well personally, but he's somebody who feels very free to publish Maya materials that have come from unprovenanced excavations. I think ultimately the main concern ought to be to try and put a stop to the looting. That means, first of all, that it's much worse to have a hand in publishing something which might subsequently be offered for sale. That is I think a very serious error because in that way one is actively contributing to the cycle of looting and sale and looting and sale. But, even by publishing antiquities that have ended up in public ownership—and the Goulandris collection is not in public ownership yet, but it may become so—one is somehow accepting or acquiescing or validating the looting process.

It can be a difficult decision when you have good reasons for thinking the objects that are available for description are genuine. You don't have to know about context always to have the view that they are genuine. So if the objects are genuine, and of unique form, then it's a considerable loss to scholarship and to knowledge not to describe them. So there are difficult decisions to be drawn there, but I think it's probably fair to say that up to now, in most of those difficult decisions, the balance



has gone in favor of the vendor and the purchaser, and I think probably more discretion would be appropriate.

SMITH: I know that you're not an art historian, but have you advised collectors who were interested in Greek neolithic or bronze age materials?

RENFREW: No, I've never done that; indeed I've declined to do so. Sometimes dealers have shown me Cycladic antiquities and asked my opinion. Initially, my diffidence was more because it's actually very difficult to know what is genuine and what isn't. Marble is a very difficult material, and many of the antiquities are of marble. So in the early days I declined to offer an opinion because I really felt I didn't know, but subsequently, I have been very clear in my mind that it is a serious disservice to offer specialist opinion on materials which are in a potential cycle of sale and purchase, like indeed the Ortiz collection. Once they end up finally in a museum from which they will not leave, then the situation isn't quite so bad, but, still, the Getty bought a lot of Cycladic material which it shouldn't have bought, and then people published it once it was there in the museum because once there I'm sure it's likely to stay. But the whole thing was rather discreditable; they would be much better not doing that at all.

SMITH: I noticed also that you were chairman of the National Curriculum Working Party on Art. What was that about?

RENFREW: Well, it was a very interesting enterprise. The government decided



some years ago that it was going to revamp education in schools, and in order to do so there was going to be a national curriculum for each subject. In each subject there was going to be a description of what pupils in a general sense should be expected to learn and by what age: the objectives of their learning, their attainment targets, and to some extent what the subject matter of their learning should be. So we spent a lot of time defining these things and reached a very good consensus with the art teaching profession. The government, or at any rate their advisers, rather regrettably thought this was too elaborate, so they simplified it quite considerably, to the dissatisfaction of members of the committee. But, nonetheless, it was a worthwhile exercise, and it certainly was undertaken in sufficient detail. We recognized very clearly that you have to have some view of the arts of other cultures and think about the role of ethnic minorities and their arts within our own community; it was a very worthwhile and interesting exercise.

SMITH: And of course those recommendations remain on public record?

RENFREW: They do. Although they were modified and simplified in a way that we weren't pleased with, they are now the national curriculum for art in England.

SMITH: I wanted to ask you about your excavations at Keros, which you did only within the last ten years.

RENFREW: They should be published by now; it was ten years ago, that's right.

Keros is one of the major looted sites of the Cyclades, which I first visited while I was



undertaking my doctoral dissertation, in 1963 or 1964. It was the Greek archaeologist, Christos Doumas, who had in fact been tipped off by Mrs. Goulandris, who had passed by in her yacht in one of her Cycladic cruises. He told me about Keros I think before he had been there himself, so I went there and saw this huge tip of disturbed soil, and in it was a huge number of very beautiful painted sherds, fragments of marble, and marble bowls. I collected several figurine fragments in the course of three or four hours; it was a really rich turning over of fragmentary material. I left the material I collected with the Naxos museum, and I used it in my doctoral dissertation. I in fact called one epoch the Keros-Syros culture because of the very well-defined character of the Keros finds. There was the strong suspicion that quite a lot of the looted material had reached the market, large scale marble and so on, might have come from there. It seemed it would be an interesting exercise to reexamine the site professionally, with proper archaeology. Professor Doumas did a small excavation there a few years ago and so did Mrs. [Photeini] Zapheirpoulou.

[Tape IX, Side Two]

RENFREW: The Keros project was a joint project, undertaken on behalf of and for the Ephorate of Antiquities—that's the local branch of the antiquities service—by Professor Doumas of the University of Athens, Professor [Lila] Marangou of the University of Ioánnina and myself. We first of all conducted very careful field walking and collecting from the surface of the entire area, which was gridded out, to



establish the extent not only of looted material but the extent to which materials were appearing on the surface. Then we dug a number of trenches in the area and sieved the materials. All that we found was very fragmentary material, but we worked on the hypothesis that the looters, when they were shifting soil in order to find more, would only throw it a meter or two, so we could be fairly confident that most of what we found had only been displaced two or three meters from its original point of origin, which I'm sure was a perfectly reasonable assumption. We recovered many fragments of marble vessels and bowls and of very fine pottery vessels, so we probably obtained a very good conspectus of much of the range of material that was there. It turns out to have been by far the richest site in the Cycladic islands, and its looting has been a colossal loss to scholarship. We still don't really know what the site was. It could be argued it might have been a very rich cemetery, or one could argue that the concentration of finds is so great that maybe it was the site where people made ritual deposition of symbolically significant objects. But certainly it was a site of very great interest and importance.

SMITH: And there's no way of reconstructing what it might have been at this point?

RENFREW: That's right. Some of the villagers took part in the illicit excavations, but you can never get a very clear account of that. It was suggested, and indeed it seemed to be the case, that they must have started at one point with a trench right across the width of the site, the bedrock being two to four feet down, and they just



dug into the side of the trench right through, all the way across. So it seems that the entire site has been turned over: there is no undisturbed material at all. It's a great tragedy.

SMITH: Would it be possible to recognize the looted material as it's turned up in collections and museums?

RENFREW: Very difficult, unfortunately. People have been doing a provenance study on Cycladic marble, which is not yet very well developed. You could see that if you did that on all our fragments it might give a profile of where the pieces had come from, if the marble characterization worked, but I expect they would have come from quite a wide diversity of sources in the Cyclades, so that you wouldn't necessarily learn very much about Keros by analyzing whole pieces in private collections. There are just no diagnostic criteria known for saying in what place something made of marble was found. So it's not easy to see how to make progress with that.

SMITH: So it is truly all lost information then?

RENFREW: A massive loss. Our investigations went some way to giving some indicators of the nature of the site, but, for instance, one of the great mysteries of Cycladic art history concerns the very large figures, nearly two meters long. Some of them may be fakes, but some of them aren't, and we don't have a single case where one has been discovered in well-documented archaeological excavations. I at one point put forward the theory that maybe there was in fact a sanctuary, perhaps at



Keros, where these almost life-size figures were used as effigies in religious cult or ritual. Other people say no, it's perfectly clear they come from graves. It isn't perfectly clear, but it's perfectly possible. So there you have two hypotheses, and others might be formulated. We have no way of deciding between them, because every piece we know comes from looting.

SMITH: I wanted to postpone our discussion of *The Cycladic Spirit* until tomorrow, because probably we both had better be fresher.

RENFREW: I should think that's probably true.

SMITH: I think this is a relatively modest question. You have mentioned [off-tape] that thanks to [Gustav] Kossinna, ethnicity has been a compromised topic for archaeologists to work in, and that got me thinking. What in general have been the interesting topics which, for political and other reasons of the moment, have been difficult for archaeologists to tackle?

RENFREW: I'm not sure, when you put it that way, whether there are other topics that fall in that category. It would be well worth asking ourselves if there are.

Certainly the ethnicity issue is a delicate one, though I think it was greatly to the disservice of archaeology that it wasn't cleared up earlier. Clearly, during the period of National Socialism in Germany up to the end of the Second World War, there were very simplified concepts: ethnicity was equated with race, which was equated with language, and then equated with value judgments in terms of worth, so that was



racism in the worst sense. I'm not sure that there are other subject areas of comparable sensitivity, but so sensitive was that after the war that the topic was abandoned altogether. That's one reason I think that the whole language issue, which is only part of it, has surfaced so late, forty years after the Second World War, though admittedly Marija Gimbutas was writing about it perfectly respectably much earlier.

The ethnicity issue goes beyond language. Whereas the concept of language is fairly well defined, we know pretty much what we mean by language, the concept of ethnicity is often interpreted in very different ways. I think it's regrettable that archaeologists and anthropologists didn't much more rapidly analyze the concept of ethnicity and realize and show that it's a matter of judgment. You decide, in a sense, what ethnicity you plan to affiliate yourself with, which doesn't mean that you aren't clearly born into a context, with a language and with your own descent; those components of ethnicity are not open to choice. But ultimately ethnicity is what a group decides is its own identity, and that is a volitional matter; it doesn't mean it's a free choice, but it's still a volitional matter.

So ethnicity is not something that is given to you irrevocably by generations of descent in the way that your genetic composition is, though one understands that genetic composition in respect of group membership doesn't make a great deal of difference in terms of various human capacities. Had this understanding of ethnicity really been spelt out more clearly and understood as a universal generality, then some



of the things that happened in Yugoslavia couldn't have taken the form that they did. "Ethnic cleansing" as a concept could not work if one had a more sophisticated notion of what ethnicity is. That doesn't mean that the war wouldn't have taken another form. Our companion at dinner this evening was more or less saying that the whole thing was a German conspiracy. He was taking a very strongly critical line and he was saying that the Yugoslav war was essentially a playing out of power interests between Germany and Russia. Well, if you follow that line the war would well have taken place whatever the concepts of ethnicity. But it's perfectly clear that for a bizarre and very anachronistic concept like ethnic cleansing to come about, what was required was not only a society like Yugoslavia that was conceptually extremely primitive, but a wider world where there wasn't sufficient clarity about these issues to expose at once the primitive and erroneous nature of these concepts. There I feel that we as archaeologists and anthropologists share a responsibility; after the [Second World War] we didn't tackle these issues and ask what went wrong. That could have happened and didn't, and that's why the confusions still remained which have allowed these terrible atrocities—the slaughter of tens of thousands at Srebrenica and so on—to come about.

SMITH: But to some degree doesn't the correlation of blood types and language, which seems to be working somewhat with Cavalli-Sforza, and with the Native American population, reinforce certain nineteenth-century conceptions of ethnos as



this entity that is in the blood and gets expressed through this metaphysical thing called language?

RENFREW: Well, certainly, if you have a good correlation between language and genetic composition, it does indeed rightly emphasize that there are hereditary components. The term "in the blood" used to sound a terribly barbaric view of thinking about the matter, but since most genetic samples are from blood, "in the blood" is in fact an extremely good metaphor, a much better metaphor than had been realized when it was first coined. So, for a discussion of those components that are hereditary and in that very broad sense genetic—whether linguistic or in the field of the genes—that is so, but a modern anthropological understanding of the notion of ethnicity, of collective identity, wouldn't put undue weight on those aspects, and that is where the confusion arises. An *ethnos* is a group of people who no doubt have inherited certain commonalities, but above all, the key determining factor is, it is a group of people who regard themselves to be a group of people and generally have a name for themselves, just as there may be names for them given from outside.

There have been in the past many groups whom we look on as groups, for which scholarly names have been offered, but who did not have such names for themselves. The Celts, for example. Now, if the Celtic problem had been better understood and thoroughly unraveled in the fifties, it would be much more difficult to talk about the Serbs in this similarly fundamentalist way. Apart from the irritation I



expressed earlier, that European prehistory was screwed up by this notion of the coming of the Celts, there is a widespread conception in Western European society that the term "Celts" means something. Well, the word can be interpreted in so many different ways, and some of them might well have meaning, but basically it is a misconception. And it's a misconception not so far from the misconception of being "fundamentally Serbian," which makes it easier for you to decide that those persons who are "fundamentally Croatian" are not your friends.

SMITH: Yes. But the term "Celt" obviously couldn't have meant anything in the third century B.C., since it didn't exist.

RENFREW: It existed in the first century B.C. The Romans and the Greeks spoke of Keltoi, or Galatae was the Latin equivalent. I would be happy to discuss with you what it does seem to have meant, but it certainly didn't at that time apply to language, for instance. If you're talking about who are the Celts today, before reaching any definition you would very soon be talking about the Celtic languages. It's a transferred epithet, really. The Celtic languages were very well defined in the eighteenth century by Llwyd, who transferred to language what had previously been broadly a geographical designation.

SMITH: Yes, though in much of the nineteenth and the twentieth centuries, the "Celtic" or the "Gallic" have been used in both the British Isles and in France in very specific national mythic and political ways.



RENFREW: That's right, that's right.

SMITH: Completely independent of language.

RENFREW: No, not independent of language, I would suggest. You'll find it has only been applied to those communities which not only originally spoke but still aspired to speak a Celtic language.

SMITH: In France, actually, there was a lot of discussion about whether people were Celtic, Roman, or German, and then, even more specifically, who was Celtic, who was German, and who was Roman.

RENFREW: Do you mean in recent times?

SMITH: Yes—who in 1896 was Celtic. It had nothing to do with language.

RENFREW: But I wonder if that's entirely so. I'm not familiar perhaps with the specifics of your argument, but I'm sure all the Breton speakers fell in the Celtic category.

SMITH: Oh, they were in the Celtic category—

RENFREW: It's just that some of the population of France had originally spoken languages which were not Roman and not German. Clearly many French people speak French, and clearly French is a Romance language which was imported from Italy, so they are partly aspiring to talk about earlier antecedents, aren't they?

SMITH: Well, yes, and it has to do also with whether or not you can assert that all the folkloric myths of a community are Celtic in origin. If you can, then you can say,



"Despite the fact that these people are speaking a Latin-derived language, they are still Celts because they hang mistletoe on the door," or they put up a Christmas tree, etcetera.

RENFREW: I'm afraid I must confess to a prejudice I've had for years. I know this is unjust and would irritate many serious scholars, but I relegate most archaeological uses of folklore to the same wastebasket as craniometry applied in the search for ethnic identity. A bit unfair, but nonetheless, that is my view, from an archaeological standpoint.

SMITH: But Gimbutas was professor of mythology and folklore at UCLA.

RENFREW: Well, at least she never resorted to craniometry. [laughter] She was indeed a specialist and I think there is value in being a specialist in the folklore of a particular area, for its own sake. Gimbutas knew about Lithuanian folklore and is very dedicated to her Lithuanian roots. So I wouldn't necessarily denigrate folklore, but if people try to make something more out of it and use it as a source of information about the deep past, then I think they are lost.



SESSION FOUR: 18 MAY, 1996

[Tape X, Side One]

SMITH: You had a few more points you wanted to make about the question of language.

RENFREW: Yes, I just thought it was worth emphasizing how attractive is the prospect of trying to find overall patterns of explanation for features of the distribution of language families. Some of those language families have very broad distributions and others are quite narrowly localized. Sometimes, for instance, in the Caucasus or in New Guinea, you have juxtaposed languages, or even language families of quite limited distribution geographically, which are really very unlike each other. The same is true in north Australia. It must be the case that the ancestors of those persons have lived in those areas for very long periods of time for so much divergence to have occurred. But you have other areas where you have vast tracts of land occupied by quite closely related languages which obviously belong within the same language family, and clearly something very different has happened there.

You were saying there's no reason at all in view of extinctions why all languages in the world should be related ultimately, and I think that's absolutely right; it's a very fair remark, unless you feel that living languages do retain buried within them some elements that go right back to the depths of language origins. Although it is still a matter for dispute and debate, it's very widely felt that all humans today are



ultimately descended from the same ancestral population in Africa; that would be perhaps towards a hundred thousand years ago. And there are some linguists, who of course are decried by the majority, who actually believe that in all languages today, or most languages, you do see some echoes of the primeval *Ursprache*. Merritt Ruhlen is one of those who has taken up this argument. The word *tik*, for "finger," or "one," is a root which he and his colleagues claim is found in very many languages and a whole range of language families, and is perhaps a remnant of the original, in that sense universal, although restricted, language. I myself am skeptical of the linguists who say nothing can be said today of languages which were spoken prior to five thousand years ago; that there's an absolute barrier. I don't particularly see why that should be so. On the other hand, those skeptics who feel that talking about vocabulary surviving from fifty or a hundred thousand years ago is ambitious, may have something there.

SMITH: How can these arguments ever go beyond the stage of speculative hypotheses?

RENFREW: I think it's very possible that they can't, because the only way that you can conceive of testing them is in vague probabilistic terms. On the other hand, if we look at the arguments which Greenberg uses for the Amerind family, he claims that these languages are related because there are certain particles that occur in a very widespread way in different languages in different families—pronominal particles and



so on—and he argues that this is far more than could have happened by chance. I suppose that there you have a fairly simple assertion which you would think might be capable of statistical test, but, on the other hand, to recognize the occurrence of these particles and accept a meaning for them in a particular context, you have to ask yourself, how close is this meaning to the supposed original meaning? In each case there are so many judgments to be made before you accept or reject this assertion that I think it is very difficult to put it in formal statistical terms. So I think it's a difficult issue.

But I wanted to emphasize that at a world level, one ought to be able to reach some understanding of the factors that establish which language family will occupy a particular tract of land. Why are some language families restricted, why are some very large, and what historical circumstances led to those distributions, given that generally, as we were saying earlier, it's assumed that if you have a language family, all the languages of that family are derived from an ancestral proto-language? I think it's fair to say the historical linguists in general don't really seek to have any very close historical understanding in terms of the demographic or social factors. Their discussion is in terms of language, and many of them are not particularly interested in talking about the people, the societies, the populations, which spoke those languages. So historical linguistic argumentation tends to talk about language in terms of linguistics, which is perfectly reasonable and logical, but at the same time it does



overlook what must have been the social realities. There must be factors which underlie the spread of languages; they don't just spread by their own volition. There must be social factors favoring and disfavoring such things, so it ought to be possible to talk about these matters.

I published an article in *Scientific American* in which I suggest that in some of those areas I was speaking about, like the Caucasus or north Australia, where you have a great deal of linguistic diversity, clearly of great time depth, the settlement probably became established already by the late Pleistocene period and hasn't been greatly disturbed since then. The large family extensions, clearly of more recent origin as distributions, are subsequent to that and many of these may indeed have been the product of farming dispersals. I mentioned Peter Bellwood, who, quite independently, working mainly in the Pacific, has reached very similar conclusions.

I think there are arguments there which may or may not prove to be well sustained, but if they are, they are giving insights into the whole global pattern of language family distribution. This is a fascinating topic, and it is one where the genetic evidence will have a bearing. Of course molecular genetics tells you nothing about language as such at all, but it does tell you about population history, and of course language distributions are partly the product of population history—expansions, movements of people, and so on. Nobody would deny that there are relationships between the two, though there's no reason to assume a one-to-one



correspondence. So there's a wonderful field there, and the exciting thing is that the molecular genetic evidence is becoming more and more abundantly available. I have no doubt that the provisional conclusions reached up to now by molecular historians like Cavalli-Sforza may well prove to be oversimplified and some of them erroneous. The present data relate only to particular areas of mitochondrial DNA, and studies of the Y chromosome are appearing, but there are almost endless loci, and each locus can give its own insights into historical patterning—all of this from living samples.

If the promise is fulfilled that it becomes routine to extract DNA successfully from ancient human remains, then you have a way of testing many of the hypotheses. At present one is making all these historical statements on the basis of the patterns now, and how those patterns, presumably by parsimonious principles of explanation, have come about in a particular way. But clearly one could have different ways of explaining that, and one could have means of discriminating between those patterns using ancient material. If we're talking about Kurgans, maybe we can analyze the remains from Kurgans of 3000 B.C. in the Ukraine and of Beaker skeletons in Europe. They won't be sufficiently numerous to give a very clear patterning, but they might confirm or disconfirm structures of descent which one had been able to infer from the present-day patternings. So it really is very possible, if we begin to get ancient samples coming through effectively, that in ten or fifteen years we will have a very detailed understanding of the history of human populations, and if we have a detailed



understanding of the history of human populations, that is bound to set very firm constraints on our understanding of the history of languages.

SMITH: I may be mixing up biological categories here, but if one were to do DNA studies from burial sites from the neolithic or mesolithic period and found evidence of high rates of rhesus negative factors, could you then presume that these were proto-Basque populations?

RENFREW: Yes, broadly; it may work out in more detail that that particular blood group is not very stable through time, although, on the other hand, if it's alleged to have paleolithic origins, then the assumption would be that it is stable through time. But yes, that sort of thing is exactly what one might show. Another example: If the hypothesis is that much of Europe was related to the populations which have survived in the Basque country, and if the Basque language is the descendent of a pre-Indo-European language in Europe, perhaps of the late paleolithic period and therefore much more widely spread, well, that has very precise predictions. You might expect to find the rhesus negative character much more widely spread in mesolithic populations than in neolithic populations after the coming of the neolithic farmers by demic-diffusion, if we are taking that argument.

SMITH: How do you factor in something like enslavement, which doesn't necessarily have to be plantation style enslavement, but as with the Native American populations, incorporation into the group one or two at a time?



RENFREW: There are two points there: one is social relations, the matter of elite dominance. Although one might find that too simple a concept, it is something you have to consider; in other words, it is perfectly possible, if you have social hierarchy, that the language of a dominant group, which is actually demographically a minority, in some circumstances can become the language of the entire group. There, as you very rightly are implying, you get a dissociation between the language origins and the genetic origins: the genetic basis would ultimately be related mainly to that of the majority, whereas the language would be related to the minority. That happens in some cases of language replacement, especially when you have elite dominance factors.

But you are also laying emphasis on something that is very important and not sufficiently well understood: issues of gene flow, and no doubt also of gradual language changes taking place over long periods when nothing very dramatic happens except a gradual seepage of population; over long time periods this development can be quite decisive in determining the genetic composition of populations. There one is really talking about population statistics. I think it is the case that we will all have to do a great deal of learning so that we begin to think in valid population statistical terms rather than just thinking of this group replacing another group partially or completely or not at all. I've no doubt that the ways we currently talk about these things are statistically very primitive. On the other hand, I think the population



statisticians do have a good understanding of the relevant factors; they perhaps just haven't succeeded in making us all think about these things in the right way. I think it will probably be the case, in ten or fifteen years time, that not only will it seem that the genetic data we currently have available are very sketchy and provisional and inadequate, so too probably will it be felt that the terms we are using to talk about these things and the models we are employing are also extremely primitive—sort of yes/no, rather than seeing the gradations that a truly quantitative approach would allow.

SMITH: Is this what you are working on now?

RENFREW: I've been wondering whether to write a book on world linguistic diversity. Obviously, I'm not a linguist at all, so I would be using the evidence that is presented by competent linguists, looking at it historically, in time depth, which few linguists have done. I would be using archaeological data where possible, and then I would also talk about the genetics, which is a field so vast that it's fair game for anybody; it's too important to be left to the geneticists, particularly if you are talking about linguistic issues also. I don't think there's any rule that says it has to be a person with this or that specialism who must write a treatment in so vastly interdisciplinary a field.

But I keep on hesitating, because I am impressed by those linguists who say that this is all just absolute hogwash. It's still the majority of linguists who say that



we can't go before 5000 B.C. and that Greenberg and all those Russian Nostraticists are completely up the creek. I don't myself see it that way, and I'm not altogether persuaded by their linguistic arguments so far as I understand them, but *they* are the linguists, so it is a little puzzling. The only thing that makes me feel that maybe they are being too cautious and too modest is that apparent support which molecular genetics, notably the mitochondrial DNA, has apparently given to the Greenberg picture. If the molecular genetics is showing that the population predictions which Greenberg is making turn out to be fully warranted in terms of the understanding of population history as derived from the genetics—and I'm not there saying that genetics equals linguistics, we have to think that through properly—well then, that would be such a massive support for Greenberg and such a setback for those linguists who say you can't go back more than two or three thousand years, that it really might make one feel that perhaps Greenberg's approach really does have a lot going for it. In which case, the Nostratic view of macrofamilies and great time depth might also have some validity.

So it's rather risky; it's quite possible I might write a book full of interesting ideas about how it all happened, yet since the starting point is the distribution of language families today, if the linguists are right and you really can't go back more than a few thousand years, I'd be writing a book the whole of which would be more or less nonsense! So it makes you pause before you start writing on page one, doesn't



it?

SMITH: Well, you remember what I told you Max Delbrück said to me, that we learn more from our failures than our successes.

RENFREW: Well, that's right, and Sir Mortimer Wheeler once wrote to me, "Don't be afraid to be wrong," which is roughly the same idea, isn't it? I've also often taken the view that if people are saying new things which are not entirely right, then they are making contributions much greater than if they weren't saying new things at all. You don't have to be more than fifty percent right in order to make a contribution.

So when I have a little time, when I've stopped occupying myself with the college council or the buildings committee or the bursarial committee or the staff committee or whatever it is, I think it would be fun to write such a book. Although Peter Bellwood may already have done it as a matter of fact, because we really do seem to think along very similar lines.

SMITH: I had wanted to ask you about the general processes by which your thinking began to shift from what I think you call a "functional processualist viewpoint" to a cognitive processual viewpoint. What were the internal contradictions that you were dealing with and what were the external pressures?

RENFREW: Right. To start with, I think I would say that I don't think I have ever really been in the functional processual category. I think the distinction you are making is generally true, that in the early years of processual archaeology the



emphasis was very much on environmental factors, and there was a good deal of environmental determinism at work, and it's only more recently that a great deal of emphasis has been placed on cognitive factors. You could parallel that, in a way, with the neo-Marxists, who proclaimed that the classical Marxists had laid far too much emphasis on the primacy of economic factors and the infrastructure and argued that the superstructure had its own role and wasn't secondary to the infrastructure. Then of course they naturally projected it back to Marx, so that in their perspective Marx himself had never really insisted on the primacy of the infrastructure, because if you are a Marxist it's inconceivable that Marx could be wrong; therefore, whatever you are saying now Marx must have originally have said, so you have the great exegesis of the texts. But still, there was a shift in direction there.

In my own work, I think it's fair to say that I have always had some interest in cognitive aspects. For instance, when I wrote *The Emergence of Civilisation*, I developed a systems approach for discussing prehistoric Aegean society, so it was logical to divide areas of study into a number of subsystems. There was a subsistence subsystem, metallurgy was given a privileged role because it seemed to be so important in the Aegean, there was trade and communication, there was social organization, but there was also the symbolic or projective system, which also could be called the cognitive system; it would mean exactly the same thing. I devoted an entire chapter to symbolic and projective systems, and though it wasn't altogether a



successful chapter, it didn't really show fully how these things would relate to the other subsystems, it certainly recognized that that issue was every bit as important as the others.

It's true that most of my earlier writings were devoted more towards social than projective aspects, but, nonetheless, in writing about the megaliths, for instance, I did try to express what their role was, and if their role was to be territorial markers, it may in some senses be functional, but it is also cognitive. The megaliths functioned as elements of communication in the society, making symbolic statements about those small communities in relation to their neighbors. I think that was, in a way, looking at the cognitive dimension. But I have emphasized cognitive aspects recently because there is no doubt that many of the early New Archaeologists were close to being environmental determinists, really. I think that can be said of many of Binford's studies, but that is mainly I think because Binford focuses on hunter-gatherers and that doesn't leave many data in the cognitive area to talk about. Others, like Kent Flannery and Joyce Marcus, have taken a very strong interest in cognitive matters. But much of the polemical thrust of the early New Archaeology was very much on the functional side, and that's why I've thought it appropriate to emphasize cognitive processual archaeology; in other words, to point out that you're not necessarily going beyond the aspirations or indeed the practice of processual archaeology when talking about cognitive issues if you talk about them in a way that is consonant with the



broad processual approach.

SMITH: Cognitive work can also have its own deterministic aspect; for instance, cognitive psychology involves how the eyeball moves about and has a Kantian aspect in terms of uncovering the manifolds by which we process our perceptions, sometimes in ways that are considered to be independent of language and culture. We have to interpret certain kinds of external stimuli in particular kinds of ways, and I suppose one could see value in taking contemporary research in how, say, the eyeball operates and applying that to a presumed visual environment in prehistoric societies.

RENFREW: That's right, and some people have started doing that. We had in the department a research student, now Dr. Jeremy Dronfield, who was developing that sort of approach and was looking at the art associated with some megalithic tombs in terms of those images which you see on the eyeball just through the operation of physiological factors, and he is very interested in developing that line of thought. I think there clearly is a whole field there which merits development, and Dronfield is very anxious to develop links with modern psychologists working on the physiology of perception.

At the same time it has to be said that, so far, much of brain science still seems some way from explaining what we experience ourselves when we are using our brains. One of the great fields of excitement in science is the working of the brain. It's very interesting that people are talking more and more seriously about



consciousness and trying very hard to see how the experience of consciousness, an experience which we undergo as subjective individuals, may perhaps be related to what we are beginning to know about the brain as an *object* of study. Obviously to relate the subjective and objective in that way is an ambitious project.

There was a very interesting discussion between Karl Popper and Sir John Eccles some years ago. Sir John Eccles, the specialist in brain structure and the functioning of the brain, was very willing to talk philosophy with Popper, and Popper of course was very willing to talk science with Eccles, but not a great deal emerged from the discussion at that time. But it's going to happen: understanding more about the functioning of the brain and how that really does affect our perception and our thought is clearly going to be perhaps the major field of science. We obviously had this great era in the sciences, over the past thirty or forty years, as a result of which we now understand the mechanisms of life, through DNA and all of that. That has brought us huge insights and continues to do so. Obviously, molecular genetics was made possible by those discoveries.

We've really come to understand the physical world as never before, and now, in our own time, we've had this revolution where we understand the biological world, and maybe we can expect, even in our own lifetime or that of younger people, a sort of neurological revolution, if that's the right way of talking about it, where we shall actually gain an understanding into the physiological working of mind. That clearly is



a major objective, but it has to be said that at the moment I don't see many insights readily on offer. Jeremy Dronfield's area of work may well be valid, but it is quite a restricted area so far.

SMITH: One of the difficulties is that the field of psychology itself is immense. The number of works produced every year is beyond the capability of any of us to read, even if we devoted an entire lifetime, but do you attempt to follow some of the developments in aspects of cognitive psychology?

RENFREW: Not yet. I'm aware that I ought to. Certainly, having already written a good deal about language diversity, it would make sense to write something which might pull together what I've already written and wouldn't involve saying very much that I haven't already to some extent said. But the theme of the archaeology of mind is one which has interested me for a very long time. I gave my inaugural lecture here in Cambridge on that topic way back in 1982, and it would be really interesting to try and sit down and write a more comprehensive work which would mainly be devoted to the functioning of symbols. Preliminary to that one really ought to be thinking more about the physical basis of mind, but, as you say, it's so vast a subject, and in those studies that I've read so far, comparatively little comes through that makes you as an experiencer, somebody with a consciousness, say, "Wow, now I see more clearly why my thinking operates this way. It's because the machinery of my brain operates this way and not some other way." I don't think, as far as I'm aware, that



we're getting many profound insights yet. It may simply be because I'm ignorant of what is written.

As I see it, cognitive archaeology definitely falls into two subject areas: one is the development of the human being, the emergence of humankind, which is generally equated with the emergence of our own species, *Homo sapiens sapiens*, which takes you up to about forty thousand years ago. Most anatomists and biological anthropologists would agree that forty thousand years ago our species, *Homo sapiens sapiens*, was pretty much fully formed and that if you looked at us today and compared us with our ancestors of forty thousand years ago, there isn't much physical difference. Indeed, they might well say that if we were able to look at the DNA, which may well be possible as we were saying, then one would find that there isn't a great deal of difference overall between the two.

So one question is, how did the human mind form, and what are its particularities? There we are talking about a formation process up to that time, which means that you are also talking about human behavior in the Lower and Middle Paleolithic. The Upper Paleolithic is more or less regarded as coterminous with the arrival of *Homo sapiens sapiens*, so then we are in the new era, the era of the operation of our own species. One of the mild enigmas which rather fascinates and puzzles me at the moment is that here we are, more or less the same people physically as our ancestors of forty thousand years ago, yet the archaeological record indicates



at that time none of those symbolic behaviors which we see not only in urban dwellers today, but also in contemporary hunter-gatherers. It may be there's just a mental confusion in my mind and that of others that needs sorting out there, but most of the discussion about the archaeology of mind tends to focus on how the modern mind, in the sense of the mind of *Homo sapiens sapiens*, come about. But if you take forty thousand years ago as your time point, nearly all the behaviors that you think of as specifically human behavior, are yet to be seen in the archaeological record, and that's a theme which very much interests me at the moment.

SMITH: Of course, one answer could be that they were expressed in materials that didn't survive.

RENFREW: That's absolutely correct, and of course many of them might have been expressed in words, which certainly don't readily survive. That is absolutely true, and that is one of the problems of archaeology, that you have to work with the materials that you have to work with. On the other hand, you do have some materials which are preserved from more recent times. I'm talking about lithic technology, which is obviously something that archaeologists have thought about and worked with for a very long period of time. But it ought to be the case, if you have completely different minds at work, that stone tools or any routine products would to some extent reflect that you have a fully cognizant sapient being producing these tools rather than some simpler ancestral hominid. So we may not be thinking about the data in quite the



right way. Because we can easily look at neolithic manufacturers of stone tools and other products, and we can dig up more as well. Although the material record of the past is a diminishing resource, happily, paleolithic stone tools are not very extensively looted at the present time, so there are abundant remains which can be more intensively scrutinized.

SMITH: *The Ancient Mind* [*Elements of Cognitive Archaeology*] is a move towards explicating these issues.

RENFREW: I was never entirely happy about the title; a title needs to be short and that is short, so in that sense it's a good title. Some reviewers have pointed out that we did actually say in the book we were not trying to express the view that the ancient mind of five thousand years ago or ten thousand years ago was fundamentally different from the modern mind, and yet the title could be interpreted in that way. But yes, that book is an attempt to present some of these issues coherently, and also to encourage discussion of them in a way that would be at least systematic, shall I say. As we were saying earlier, to start banding the word "scientific" around doesn't really mean very much since the term has favorable overtones to some and unfavorable overtones for others, who think of some sort of Gramscian, "scientific," hegemonistic takeover of the world.

But, nonetheless, yes, there are a number of archaeologists who feel that one ought to be able to develop procedures that, so far as possible, get beyond the



limitations of entirely subjective vision. After all, that is very much what science is about. I'm talking now about hard science. I'm not claiming that what is being done in that book is science, but there is the scientific objective of making repeatable observations. Of course that is more difficult when you are dealing with the past; you can't rerun experiments, you can't have fundamental particles jumping about before your eyes, because that's not the subject matter you are looking at. But one hopes to use procedures that avoid centering themselves exclusively in the subjectivity of human experience. There are interpretive archaeologists who would take the opposite view and say that we experience things as individuals, and our experiences are subjective, so let's admit that, let's lie back and enjoy it and make the most of it.

SMITH: You're talking about the postprocessualists?

RENFREW: Some of them, yes. But equally Jacquetta Hawkes, who wrote a book where she developed that to what I consider an extreme degree; she more or less asserted that she did have direct experience of past ages, which gave her a particular authority in her writings and was very convenient. I have to say that some of the new geographers of the "sense of place school," write about putting yourself in other people's shoes, as if it's not an unimaginable and impossible task to get into other people's minds. This is all part of a long idealist tradition. [Robin George] Collingwood was one of those in Britain who advocated this. He said if you want to know why Caesar crossed the Rubicon you have to make yourself be Caesar. He



traced his own line of thought back to [Benedetto] Croce, for instance, who said, "If you want to understand a blade of grass, be a blade of grass." Well, I've always thought of this as an inappropriate research strategy.

SMITH: I was going to ask you to distinguish cognitive archaeology from post-processual, since they both deal with the question of the symbolic projective, or the semiotic.

RENFREW: Yes. When you get down to it, I think some of the differences may be more apparent than real. I don't use the term postprocessual, because, as I mentioned earlier, it seems to suggest a rather chronological absolutism about these matters. Some of the so-called postprocessualists are entering subject areas which have been neglected by some archaeologists in recent years, which I think is very worthwhile. But there is one strand in postprocessual writing that deliberately sets out to reject any notion of scientific method. One could of course argue what "scientific" would mean in that context, but they set out to reject science for no very good reason that I can see. Some of them, such as Michael Shanks and Christopher Tilley, develop what one might regard as an instrumentalist view of the discipline of archaeology, to serve certain social ends, and therefore archaeology *should* reach conclusions of this kind or that kind. Now, given that they've also embraced subjective approaches in various ways, that pretty well guarantees that they can reach conclusions of that kind.



[Tape X, Side Two]

RENFREW: There is something of the past "as wished-for" in the writings of some of these postprocessual archaeologists. To my way of thinking they do not sufficiently respect that our knowledge of the past has to a large extent to come from the data which we recover. Some of them, in their more extreme writings, tell us that the data are not objective, which in some ways is a very fair observation, but that leads them sometimes thereby to diminish the significance of the data. As an archaeologist I am clear in my mind that it is our role to find out what happened in the past by studying the archaeological remains and then to begin to understand what happened, so far as we can, by taking a maximal input from the data relating to the past. I quite accept a number of the points they make: the data are not objective, they have to be recovered by human research, which has its own motivations, there is an interrelationship between theory and data; I think all these observations are valid, but I don't think that need detract one from the main objective of using the available evidence to guide us in our understanding of the past. I really feel that some of these writers lose sight of that objective, and in the end they do run the risk of being guided exclusively by their own motivations.

I think it's a very interesting exercise to turn this argument around and say that those persons who claim they're thinking objectively and they're being guided by the data are simply fooling themselves; they too are creating a past as wished-for, but



they don't realize the subjectivity of their own frameworks. Then this could be linked with political objectives by saying, "Oh well, imperialists have set up thought frameworks to justify their imperialism, and they've done so so successfully that they don't realize that this is what they've done." Therefore it is the duty of these very insightful postmodernists to show this is the case through inspired deconstruction. I think there is a lot of validity in all of that, but the question is, what do you do with it? Do you just say, "Oh dear, all our reconstructions of the past are rooted in our own present existence and in our own subjectivity, so it's each to his own, and anything goes," which is the criticism that has been levelled against them, I think quite justifiably. Or do you say, "We accept these criticisms as having some validity, so what can we do to ensure that we are, so far as possible, allowing the data from the past to guide our present thinking about the past?" I think there really is a difference in objective there, and I think these people in the first category ultimately misguide themselves. They talk themselves into a corner where they are in a formal sense in just the same position as the people with ley lines and flying saucers and good vibrations from stones. It's possible that they are constructing a past as wished-for without seriously considering any guidance from the data.

SMITH: Well, accepting that data are theory-laden, what methods then are suggested for working through that to arrive at results that you can say are not entirely subjective? Which has to do with a definition of what is objective, as well.



RENFREW: Indeed it does. I think some of the techniques and arguments which have developed over the past twenty years or so, partly through the work of postmodernist thinkers, are entirely valid. It is right to make ourselves more aware of our own position as observers and to understand better how it is that we come to be where we are, and to some extent how it is that maybe we come to have the opinions that we have, the research interests that we have, the orientations that we have. It is very useful to see how the past has been used politically in a whole range of ways, and to ask ourselves therefore whether we knowingly or less consciously are doing the same or are part of a similar process; I think all of that is indeed helpful, and that is, in a sense, probably all that we can do.

The data are theory laden. Well then, that is worth examining: what theory is burdening the data? You see, one may say the data are theory-laden, but that isn't an entirely satisfactory statement. For instance, if I have preconceptions about the chronology of a particular monument and my theoretical orientations lead me in a particular direction, and if somebody challenges my chronology, then with radiocarbon dating they could take some samples from a particular stratum and date them in the laboratory. Now, you don't tell the laboratory what date you are accepting, and you get a radiocarbon date back from the laboratory; so that determination isn't terribly heavily theory laden. Of course, if you're one of those religious fundamentalists who says the world was created in 4000 B.C. or thereabouts,



you will have a subsidiary argumentation that the whole of modern physics is theory laden and is somehow mistaken when it comes up with dates more than four thousand years ago. In that sense you could say the data are theory laden, but if most of us aren't saying that, we could still have other argumentations that those persons who are saying these are very old have tended to choose radiocarbon samples in a particular way, or when they get a date from the radiocarbon samples they will say, "This date is clearly erroneous, I see it now. We did not choose the sample well for this and this and this reason." Indeed, if you look at the early editions of the periodical *Radiocarbon*, you'll see that in those days they asked the person submitting the sample to give an estimate of the date and a statement about the date of the sample before it was analyzed, and then they asked for a statement about the date of the sample after it was analyzed, and very often there was a considerable disparity. Regrettably, they abandoned that procedure and now only have a single comment from the excavator, which therefore always has the wisdom after the event without the clear statement beforehand.

In a way, if you are taking the thesis, "the data are theory laden," you could analyze the volumes of *Radiocarbon* and show how people were changing their position in the light of the radiocarbon determination, but the point I am making here is that the radiocarbon determination came through, the number came out of the machine, and that number wasn't particularly in itself theory laden. Quite the



contrary, you are waiting to see: *L'homme propose, la nature dispose* is the view of the hard scientist. You may have your theories, you then submit them to testing, and out pop the numbers, and it either helps to support or it refutes your hypothesis. Well, that was what was working and continues to work with radiocarbon dating. Though it can be argued that your selection of sample may be somewhat governed by your presuppositions about what the chronology is, if you have two archaeologists with rather differing views on the chronology, it's all subjected to a bit of criticism, and ultimately the chronology gets sorted out. And that is indeed what has happened with the chronological framework for Europe. Yes, the data are theory laden, nonetheless the data still speak, and their speech can to some extent transcend the theoretical preconceptions with which the collection of those data was surrounded. I am arguing for a kind of independence and autonomy that is still there within the data. The data are not subjectively generated in their entirety.

SMITH: Maybe because of my realist predilections I assume that theory laden means that the data are created through the theory, that is, your theory tells you where to go look and gives you the methodologies that allow the data to come into being as something you can observe.

RENFREW: Right. But it doesn't necessarily determine the entire content of what comes out. Some of these relativists who say the data are theory laden would appear to imply that the entire content springs from the theoretical orientation.



SMITH: Radiocarbon technique certainly is theory laden vis-à-vis physics, but the theory it carries vis-à-vis physics is totally irrelevant when it comes to its application in archaeology.

RENFREW: I think that's exactly right. So although the statement "the data are theory laden" still is a valid one in many ways, once you begin to deconstruct that statement somewhat, then as you rightly say, we are talking about different bodies of theory which are pretty much unrelated to each other. The physical data relating to radiocarbon analysis are very different from the theory relating to the prehistoric communities which we are trying to date.

SMITH: But if I understood you correctly, when you talked about your very early thoughts about archaeology when you were a student at St. John's, you were already skeptical about the diffusion thesis, and perhaps you were aware of some early radiocarbon datings, but it sounds like radiocarbon evidence was very tentative at that point.

RENFREW: Yes, it was, it hadn't really very much come to the fore.

SMITH: So you already had a prototheory, in a sense, that made you skeptical, or made you aware of the theory-laden aspect of the diffusion thesis.

RENFREW: Yes, I think that's true. I have always been of a skeptical turn of mind, and when we were talking earlier I mentioned how I think that was reinforced and encouraged by good schoolmasters and good teaching and good school experiences.



To give you another example, around that time, certainly before I started doing archaeology seriously, I read some of the writings of Jung and was at once moved to massive incredulity by some of what he was saying. His notion that early experiences would be transmitted genetically so that you would get children suddenly speaking ancient Greek—I mean, there are tested cases where people have suddenly started speaking a language which they didn't know, but Jung was seriously suggesting that this was the result of some experience in an earlier generation, several centuries back. Since reading that I've never been able to take Jung seriously for a moment, though I realize that some things he wrote may have been less bizarre.

I was also skeptical politically. I think one reason I never responded well to the socialist argumentation of that time was that some of the broad frameworks on offer were just clearly not really very well founded, or so I felt. So the much more pragmatic approach of Bow Group Conservatism, which didn't make such great programmatic pronouncements, seemed to be much less open to refutation. That is clearly an aside there, but, yes, I think it's much easier to see that something is a load of hokum, like all that Jung stuff, than to say anything more positive. I've always found it much easier to say, "Hmm, I'm very doubtful about that," than to make a clear proposition of a statement of understanding or belief which is valid in itself.

Certainly, as you were saying, as an undergraduate, after reading Childe's diffusionist writings, particularly his statements about the west Mediterranean, when I



looked at the evidence it really didn't seem very persuasive. And when I read *The Aryans* . . . well, I don't think anybody could read *The Aryans* today without saying, "Dear oh dear, there's a lot of doubtful stuff here." If you feel that way, then that can very often make you deeply suspicious of the central proposition. Now, it's perfectly true that the central proposition can be valid even though some of the arguments adduced to support it are not valid. But I think I have always been of a turn of mind that if I read a lot of arguments supporting a proposition, and the arguments are clearly invalid, then I get to be very doubtful about the central proposition, even though it is indeed sustained by other arguments whose invalidity has not been demonstrated.

So, reading Childe's book, *The Aryans*, I could see that it didn't really carry conviction, and the same was true with some of this beautifully broad picture of the irradiation of European barbarism by Oriental civilization. Indeed, when I came to study the Cyclades and I looked at the evidence in detail, it did not support the statements being made. Radiocarbon dating became applicable just a little later than that, and it did indeed confirm the skeptical approach in these matters.

SMITH: Let me ask you a little bit about some of the readings that you have mentioned. You have mentioned that you considered the neo-Marxists.

RENFREW: Maybe a little later. I haven't really sat down to review what my reading was, but there is no doubt that because I came rather late to archaeology



professionally, in my third year as an undergraduate and therefore in the year 1960, it did mean that naturally I had quite a good knowledge of science and the philosophy of science. I had done two years of a science degree, and I had done history and philosophy of science as one of the components of that science degree. I was interested in these things, and as I mentioned to you, when I was in Paris I had heard talks by Robert Oppenheimer or Louis de Broglie, or whoever it may be. I had broad general interests, so I had by then read some of Marx's writings, and some works on Marx, and I found some of the points very informative and found nothing to disagree with in some of them, although I have always thought of "contradictions" as being a very poor explanatory mechanism. That everything is to be explained in terms of contradictions, whatever they may be, always seemed to be recognized on an after-the-event basis. You can't recognize the contradictions until they have produced their dislocations, and you then say "Oh, that was the contradictions." That always seemed to me a totally circular form of argumentation.

The neo-Marxists didn't really come to my attention till rather later. They came into archaeology through the writing of Jonathan Friedman, and also Mike [Michael J.] Rowlands, who advocated Friedman's work very vigorously. I saw some merit in their statements, but not always in the polemic accompanying them. Just as I was saying, some aspects of so-called postprocessual archaeology I think are very informative, though some of the accompanying polemic seems to me based on



misunderstanding sometimes. I don't think I was directly influenced by neo-Marxist writing. It is of course the case that the influence of Marx not only operated through Gordon Childe but also to some extent through people like Julian Steward and other American anthropologists of whom I became aware.

As I said, I've never really had any formal training in social anthropology and wasn't really much exposed to it. I've never really come to terms very much with the British school of social anthropology. The American school was always more closely linked to archaeology, because the two disciplines are not separated in the United States as they are in Britain. The British school of social anthropology is often rather rarefied, or so it seems to me, and caught up in the analysis of kinship systems. You can't even read and understand much British social anthropology unless you've read a lot already, because they're not addressing themselves to the issues in any recognizable way; they are addressing themselves to each other. I find it's terribly hermetic, and I can't really understand what they are talking about half the time.

SMITH: Now, when you use the term neo-Marxism, the first names that pop into my mind would be E. P. Thompson or Raymond Williams.

RENFREW: I'm no historian of neo-Marxism, but it's really the French school that Jonathan Friedman and Mike Rowlands drew on. I wouldn't have thought Raymond Williams in that sense would be a neo-Marxist; he is simply somebody inspired by Marxist writings to some extent. I don't think he drew on Althusser, or members of

1. The first part of the paper discusses the importance of the study of the history of the United States. It is argued that a knowledge of the past is essential for a full understanding of the present and for the development of a sound policy for the future. The author points out that the study of history is not only a means of acquiring knowledge, but also a means of developing the ability to think critically and to make sound judgments.

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the postwar French school, which is how I am understanding the term neo-Marxism. Raymond Williams was a much respected fellow of this college by the way, but I only knew him slightly in his old age. He died two or three years after I came to this college.

SMITH: You mentioned Jung. What was your exposure to Freud, who was very dominant in the fifties?

RENFREW: I think it's fair to say that as a schoolboy and as an undergraduate I read Freud fairly widely. I read the good Penguin series of writings of and about Freud, and I'm sure my exposure to Jung was the Penguin book on Jung. But, again, all this reification of particular concepts, like the ego and the id . . . I tried to take it seriously for a while, but in the end it did seem to me that the conceptual framework lacked plausibility. Quite recently, I have been influenced by Ernest Gellner's splendid book [*The Psychoanalytic Movement: The Cunning of Unreason*]; he's not so much commenting on Freud but on the application of Freudian psychology. It was only published six or seven years ago, a wonderful book in which he pokes fun at the Freudian cults. Freud made an enormous contribution by examining aspects of human experience, particularly human sexuality, the way early family relations are crucial to the formation of our psyche, the importance of childhood experiences, and the way there are irrational components in our emotional being and behavior that can be approached in this way. This was clearly pioneering work, and to be able to talk



effectively and in a sense scientifically about sexuality at all at that time was a great achievement, so one can see Freud as one of the great figures in intellectual history. He was breaking ground that had to be broken.

But all this doesn't really lead one to evaluate very highly the actual content of his writings, and clearly some of Freud's writings led to rather bizarre conclusions. There has been much written about it which I am only dimly aware of through reading the Sunday papers. As I say, certainly it's rather difficult to take very seriously his categories, like the ego, the id, and the libido. These are theoretical entities which certainly had their role in allowing Freud to develop a body of theory, but I don't find that body of theory interesting or helpful, even though I agree that he developed an area which is clearly central.

SMITH: Where might one turn to to get a theory of sexuality in the family and the irrational that might be useful in interpreting the archaeological record?

RENFREW: I honestly don't know at all. Perhaps more significantly, because it's an interesting question, I haven't really asked it myself, and I really wouldn't know where to look. I'm not familiar with the writings of behavioral psychology or perceptual psychology, the sort of neurophysiological side. I'm well aware that's a completely different school of psychology from the psychology that's trying to analyze through human experience the way individuals behave—the Freudian psychology school and the whole field of psychotherapy. I'd be very interested if you could tell me if there is



a work which recognizes that Freud asks some of the right questions and develops some of the important ideas about the origins of our own behavior and emotional patterning and so on. I suppose, as so often, one would be looking at writings in psychopathology that have validity, just as, in a parallel, we learn so much about the functioning of the brain from its dysfunctioning. Books like [Oliver Sacks's] *The Man Who Mistook His Wife for a Hat* show how dysfunction seems to tell us so much more about function in the physiological aspects of human perception, and that seems to be true also in behavioral aspects. It's the psychopathologies, some of which seem to have physical origins also, which seem to tell us so much about the successful or effective functioning of the human psyche. But I honestly don't know where to turn to. If there is a *Sophie's World* kind of book in the field of psychology, I'd love to read it, to obtain some guidance. I must say, like many others, I am one of those who live in a world where Freud showed the importance of some these things and was valid in his direction, but I've never read anything that would replace, for the reader, concepts like the id and the ego and the libido and all those entities which Freud used. I'm not aware that they have been superseded by more useful entities. So it's a very interesting question, and I'm sure that that too is an interesting field, I quite agree.

In response to your comment, it's very interesting indeed that the field of gender archaeology is one that is developing strongly, although I have felt that it's been very much in the wake of gender studies in other areas. I'm not sure it's said



anything much that wasn't already available in gender studies that have developed elsewhere. But interestingly enough, when I look at the writings in gender archaeology, I've never got the slightest indication that they had drawn on Freudian theories of sexuality or theories of formation of the emotions and the emotional basis for human behavior.

SMITH: Have you had feminist research students?

RENFREW: Yes. We've had them in the department. I've not supervised them myself, mainly because they have chosen to be supervised perhaps by Ian Hodder or others who have been influential in developing the postprocessual approach. But I've had discussions with them, and in general I've respected their motivation. I haven't found many insights which have surprised me in the sense of telling me things that I didn't partly already know. I haven't had surprising conclusions offered.

SMITH: What about gay students perhaps attempting to apply queer theory to the archaeological record?

RENFREW: Right. I've had one or two of those lately who have indeed been thinking in those terms, to which I must say I would be entirely positive and responsive, but I haven't really read any assertions that are making coherent and positive statements about the past. It's a field that I would like to read more about, and I think I would be receptive to interesting theoretical statements in that direction.

SMITH: In the fifties, and actually into the sixties, the other ubiquitous intellectual



trend was existentialism. I presume you must have read a little bit of Sartre and Camus?

RENFREW: Yes, that's right. When I was in Paris, in 1956 and 1958, it was still the tail end of the existentialist era, so one could go and sit on the steps of St.-Germain-des-Prés and see people looking thoughtfully into the distance and hear wistful guitar music—all slightly like the atmosphere of blue-period Picasso. So, yes, and I found that in many ways very attractive. I do see that it's valid to seize hold of personal experience and use that. I've always greatly admired Edith Piaf, and one of my sorrows is that I never saw her perform. I realize we are talking about more elevated fields of philosophy, and most serious thinkers would think Simone de Beauvoir at a higher level than Edith Piaf, but I have to say to you that Edith Piaf means more to me than Simone de Beauvoir. Nonetheless, I would have thought that what Edith Piaf was talking about was the validity of personal experience in a way that wasn't out of sympathy with the existentialist movement.

I really do personally agree that one has to get into things and enjoy things, and that one has to be motivated to a large extent by one's personal experience and personal feelings. So, in a way, you might think I am agreeing with some of the post-processualists, and yet the rather arid and verbose and highly philosophical approach of some postmodernist thinkers seems to me often to be very joyless in a way that some of the existentialist writings were not. It's true that the writing of Sartre isn't

always a bundle of fun, as it were, but the work of Camus nonetheless is rooted in deep, strong experiences, and that I respect. I think we do have to operate in the modern world as existent and perhaps in that sense existentialist beings. So I think at a gut level I have a lot of sympathy with that view, and that is in many ways how it seems to me natural to operate. Quite where that takes one philosophically I'm not sure; indeed where it took them philosophically I've never really understood. I've never sat down to think more carefully about existentialism and where it leads us.

SMITH: Did you do any reading in [Martin] Heidegger?

RENFREW: No, although I did read and enjoy Hannah Arendt's *The Human Condition* some years ago. I have to admit that my philosophical reading has suffered. I think it's fair to say, from the British educational system, which at school rigorously excludes the direct consideration of any philosopher. I don't think while at school I was ever led to read Plato, or Aristotle, or Bacon or any later philosopher you could care to mention at all. When I went to Paris in 1956 it was extremely refreshing to find myself among French contemporaries, all of whom were at a certain level well read. If you did the French baccalauréat you knew who Descartes was. In England you might know a little about Descartes as a mathematician, somebody who contributed to the algebraic notations which we would use in mathematics and physics, and you would know something of Newton as a mathematician, but it was in Paris that I began to read a little of Plato and Aristotle and other Greek philosophers.



I think it's very sad that the only people that are taught anything about pre-Socratic philosophy in Britain, as far as I am aware, are people who are studying classics. You have to have opted that your life is going to be in the classics and you are studying Greek and Latin and then it may be indicated to you that there were these pre-Socratic philosophers—or, of course, if you are in a philosophy department. Perhaps if you are studying English literature you would be gradually guided towards thinkers, though I'm not at all sure of the truth of that remark; you might just be led to F. R. Leavis, to whom we were referring earlier. So if I have read anything of existentialist writings it's because it seemed natural to do so when in Paris in 1956.

Again, my knowledge is at a rather superficial level. Just as my knowledge of Freud or Jung comes from the Penguin books of the day—which are perfectly respectable but perhaps not very definitive sources—so my knowledge of existentialism would come from the *Que sais-je?* paperback series in France, which is more or less the level of Penguin books in this country. Perhaps for that reason I haven't got around to reading Heidegger, which is sad, really. There may be some archaeologists in the Anglo-American school who have read more widely, but I'll bet Binford, for instance, has not read much Plato or Aristotle, and I'll bet he's not really troubled himself much about Sartre, Simone de Beauvoir, or Heidegger. This would also be true of most scientists in this country, except some who have an exceptional interest in these areas, but more particularly, it would apply, I'm afraid, to many of the



so-called postprocessual writers who sometimes claim to come with more profound theoretical perceptions. I assume they have read the writers to whom they refer, [Michel] Foucault, or [Jacques] Derrida, but very few of them come to these writers with any appreciation of philosophy. They won't have read Hume, they won't have read Berkeley, and if I've read Hume and Berkeley it's because I have managed to do so a little in my spare time. I did once read Bertrand Russell's *History of Western Philosophy*, and few archaeologists I'm sure have done that. Recently I read [Jostein Gaarder's novel] *Sophie's World* with great interest and appreciation, but I have to admit that many parts of it were news to me; it was sort of remedial reading, like school children that haven't really learnt the alphabet well enough.

SMITH: *Sophie's World* might be of dubious interpretive quality, but that's an aside.

RENFREW: Well, so some would argue, but I have heard competent philosophers say they are impressed at how much the author has squeezed in, though I think they are more impressed at the sales figures and the implied royalties. [laughter]

SMITH: When you were an undergraduate and a research student here, Cambridge was a center for the application of language theory to various different fields, such as anthropology and history. In terms of the fields I know best, the main figures would be [Peter] Laslett, [J. G. A.] Pocock, and [Quentin] Skinner, but I think it was actually quite widely permeating through the university. Was that entering into archaeology at the time in some way?



RENFREW: Well, I think so. You are thinking about [Ludwig] Wittgenstein, no doubt?

SMITH: And [George E.] Moore, yes.

RENFREW: In a broad sense, the Oxford school, A. J. Ayer and so on, as seen from a distance, is part of the same British interest in linguistic philosophy. Yes, I think that was influential; it was part of the climate, and of course it relates very much to what is nowadays sometimes regarded as positivist thinking, and indeed therefore it's related to Popper. His position on the philosophy of science is perfectly commensurate with the skepticism of the earlier Wittgenstein, isn't it?

SMITH: That's a debatable question.

RENFREW: Well, we perhaps should debate it; I would be interested to be informed in the matter. But certainly, yes, I think if one was trying to read widely, first of all that was part of the atmosphere. Secondly, I think there's no doubt that some of the advances in physics—Einstein to start with, with relativity, but also in the field of fundamental particles—very much arose from skeptics saying, "Well, what would you mean by 'mass' or 'velocity'? How would you define these things?" In other words, a kind of linguistic nicety, which relates to conceptual nicety, was one of the things that allowed Einstein to examine more clearly what was meant by space and time, and it allowed people like [Paul A. M.] Dirac and others to think much more carefully about wave theory and particle theory, and those sort of things. Now, admittedly,



when we are talking about Einstein and Dirac, they were then able to follow up those insights with mathematical formulations which sprang from more particular definitions. But I think that whole outlook was very much in harmony and sympathy with the scientific thinking of the time and vice versa.

[Tape XI, Side One]

RENFREW: As I say, I'm not any specialist in these matters, but in [Paul K.] Feyerabend's *Against Method* there's an element of throwing the baby out with the bath water: the notion that some areas of inquiry are meaningless if you can't make statements whose validity can be demonstrated. If your only criterion of meaning is the criterion of refutability, then that involves throwing a lot out; it's interesting that the developments in the philosophy of science that now criticize such a statement as ultimately rather positivist herald the postmodern movement in some ways. So one can see that whereas the linguistic approach was very much in step with a strictly scientific view of the world, science itself, or at any rate the philosophy of science, has moved on a little from that.

I've always been acutely aware that the practice of science is one thing and the philosophy of science follows in its wake and sort of splashes about, and I've always been skeptical as to whether the pronouncements of the philosophers of science have ever been noticed at all by scientists themselves. I am interested in the philosophy of science, but I don't imagine that those who are working on the forefront of science,

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whether it's experimentally or conceptually, are spending much time reading what the philosophers of science say about how they should do their work.

SMITH: Probably not. Maybe that's a good thing too.

RENFREW: Well, it certainly gets out of some potentially circular arguments, that's right.

SMITH: I wanted to move into *The Cycladic Spirit*, which is a different kind of work for you. You are still clearly a social scientist, but you are also a person who's more than superficially knowledgeable about art in the modern sense—contemporary art, certainly. I don't know what your art history background or training is.

RENFREW: My art history training is very limited. Perhaps almost nonexistent. But I have seen a lot of art. I've seen several times most of the big galleries of Europe and many of those of the United States, and when I was in Paris, before going up to university, I spent most of my time, really, looking not only at the Louvre, which I got to know very well—paintings as well as antiquities—but also the museum of modern art, which was at that time at the Palais de Chaillot, and the Musée de la Ville de Paris, which was at that time in the Petit Palais. I certainly got to know the so-called modern movement in art, from Cézanne, Picasso, and so on. I did go to a whole series of very good evening lectures when I was in Paris, presented by M. Claude Ferraton. He was a very mannered lecturer, which made him entertaining to listen to, and he lectured very well about Cézanne, for instance. I think I did get to be quite



informed about the impressionists and the postimpressionists, and indeed about the analytical Cubists and the synthetic Cubists. Those lectures, as indeed have most lectures since that I know of, rather petered out after that period of art history.

Kenneth Clark's television series *Civilization* flopped totally once it got to the twentieth century. It was brilliant up to the French Revolution and a little beyond, and then he clearly had nothing to say about the twentieth century, except it didn't seem to him to be as good as previous eras. He felt the twentieth century was a period of decline, and that was about all there was to say. I looked quite carefully at Robert Hughes's *The Shock of the New*, and he was good on the time before our own, but I never felt he was totally sympathetic even to early abstract art. I saw the Mondrian exhibition recently at the Museum of Modern Art in New York, and I thought it was wonderful. I had come to have some perception of what Mondrian was doing before that because he has been very well written about by many people. But I didn't feel that Robert Hughes really got very far beyond Kandinsky and Mondrian, and he had very little insight to offer about the art of midcentury or later. I say that because he clearly didn't speak about any of it with great commitment and enthusiasm. Just as Clark petered out with the twentieth century, so Robert Hughes I thought petered out with the mid-twentieth century.

In terms of my own experience of art I have looked at a lot and certainly I think of Picasso, for instance, as an old master. When we had a very big Picasso



exhibition in this country—it must have been about twenty years ago now—there were wonderful analytical cubist paintings and synthetic cubist paintings, but it was indeed like looking at an old master. I am, like many, rather nonplussed by the view in Britain that Henry Moore is a modern. Well, of course "modern" no longer means "recent," it means "ancient," and so we use the word "contemporary" when we mean "recent," and no doubt one day we shall all be "postcontemporary." We are all postmodern, and we shall all be postcontemporary. [laughter] Goodness knows, the Pont Neuf is the oldest bridge in Paris, and the moderns are now the old masters, so there will be terrible terminological problems if we try to move with the times.

So, yes, I am very much interested in contemporary art, and do try and look at it. Although it may be that some of my appreciations of it are a little restricted because I do look at it more than I read about it. For that reason I think it took a little while before I began to see more clearly the point of some developments in conceptual art, or so-called minimal art, although, for instance, I like William Turnbull's work and you could in some senses describe him as a minimalist. On the other hand, minimalists like [Donald] Judd are doing something quite different, which is ultimately minimalist in a conceptual way. Carl André, also, is minimal in a conceptual way rather than simply in a visual way.

SMITH: But you do seem to have eclectic tastes. You have minimalist, you have what fits into post-1945 abstraction of various types, you have pop art, and to me



your parking meter garden is a combination of pop art and conceptual art.

RENFREW: Yes, that's true. Well, I've had the good fortune of coming into really close contact with a number of artists in recent years, partly through being in the college, and being part of a movement—admittedly limited—in the college to attract contemporary artists. Our exhibitions have featured a very good variety of artists, and through that program, for instance, I have come to know Richard Long very well personally. I met him first in Southampton. I should mention that in Southampton we came to know a very talented artist, Ray Smith, and he knew personally and indeed introduced into Southampton a number of artists like Richard Long and David Nash, whom I met at that time, and have come to know much better subsequently.

SMITH: David Mach?

RENFREW: Nash. David Nash is a sculptor in wood. I could show you some of his sculptures and drawings. He is undoubtedly one of Britain's leading sculptors.

David Mach I came to know more recently. I had the opportunity of getting to know him through inviting him to take part in exhibitions in the college. There's no doubt at all I think that if you know the artists it does help you to have a better understanding of their work. In fact, and this is a point we may come to, I've always been very impressed by the parallel between the position of the archaeologist looking at ancient artifacts and trying to make inferences from them, perhaps trying to seek meaning in them, and the position of the modern viewer in an art gallery looking at contemporary



works. Initially, you don't always know how to look at modern works because there are so many different ways of seeing them, and that puts you in a position very analogous to archaeologists looking at a whole body of artifacts which they feel are trying to tell them something, or at any rate there is the potential that they might learn something from these artifacts. Indeed, that is what the whole development of archaeology has been about, particularly prehistoric archaeology: progress in interrogating the artifacts in various ways, including the scientific techniques of dating, so that you either get information from them, or perhaps more accurately, you are able to make statements which seem to match up and are not refuted.

Also of course, interestingly, there is a parallel there with postmodern thought, where postmodernists say the meaning you ascribe is not the meaning that is there, or to some extent it's ourselves: we are the people who are doing the thinking and the meaning. Well, that is a view very much in harmony with the philosophy of science of the fifties and sixties: as I said earlier, *l'homme propose, la nature dispose*. In a way, that's a very postmodern statement. I hadn't really quite seen the close parallelism so clearly until formulating it here and now. In the same way there is a very close parallel between the position of somebody looking at the artifacts of the past and somebody looking at the artifacts of the present, which are the product of contemporary artists. Indeed, those two works on the piano, a cast of the Louvre head and a head or mask by William Turnbull—



SMITH: It's like a Cycladic head.

RENFREW: Well, that can be argued, and that may be why I like it. There is no doubt that William Turnbull has seen Cycladic work. I know William Turnbull's work very well, and I have no difficulty in responding to it, but when we had an exhibition of his work in the college, many people would look at it with no comprehension of what he's trying to do. To most people in Britain, and indeed in the United States, rather sadly, good contemporary art tends to be an art which in its day has a minority appeal. It is gratifying that today you have to queue up to see Cézanne and Picasso exhibitions; it may simply be that it does take a while for what is avant-garde no longer to be avant-garde, I suppose.

Certainly I find that if I go to a gallery of modern art and see the work of an artist I haven't seen before, I may start with incomprehension, and then if I'm lucky I begin to have a feeling for what is going on. If I am very lucky that feeling comes through the eyes, rather than through exegetic texts accompanying the work. Take an artist like Richard Serra, for instance, a sculptor whom I very much admire. In the beginning I didn't understand his work, but in his case if you see just a little of his work, you begin to have a feeling for what is going on without reading very many accompanying texts. I think one of the weaknesses of some contemporary art is that very often it isn't speaking to you as art, it's partly exemplifying an accompanying text, and it isn't being asserted that the text is the art, it's being asserted that the visual



works are the art. Indeed, it seems to me unless you are immersed in the writings of Joseph Beuys, or the writings about him, it's difficult to have much understanding of his work.

You were asking me about my formation in art history. As I explained to you, it was not really very much text led. But in some ways I don't see that as a cause for apology. If the art is working well it should be art led; and it has in a way been art led. Also, though I haven't explored this idea, as we're speaking, I see a little bit of an analogy between understanding art and the whole issue of language acquisition. It's always one of the mysteries of human existence that we learn language through hearing language and seeing language used. We no doubt have deep Chomskian potentialities, but it's agreed nonetheless that we don't know a word of a specific language before we are born—pace Jung, who of course teaches the contrary. We enter into meaningful dialogue with other persons through the vehicle of language, and yet in a miraculous way we don't have any prior knowledge of those words. I wouldn't want to push the analogy too far, but it is through prolonged contact with the work of an artist that one begins to feel one is understanding that work and achieving some experience through it. There is an analogy there with the prolonged exposure to a particular language that allows one to communicate in that language.

SMITH: You are getting into a major debate that's been raging for the last twenty years in the contemporary art world, which has to do with how one approaches the



object that one doesn't know anything about. There are those who think that that object engages the cognitive processes and that allows you to see those processes in action.

RENFREW: I'm not familiar to the extent that I would like to be with that debate. It's a defect that one doesn't know what's been written, although it's also sometimes no bad thing to have had a certain set of experiences oneself which allows one to some extent judge for oneself what one then reads. If you had a short reading list, which you give to your students I would be very privileged to have access to it. I would like to think more about symbolic, visual works and write more about them, and I'm sure this particular debate, which I am largely in ignorance of, would be very informative.

SMITH: The other side of it is a psychoanalytically inflected position, which is that the object becomes a screen onto which we project our desires and hence our ideology. Or perhaps as an extension, the object becomes theory laden.

RENFREW: Right, in a way that's what I was talking about in my book *The Cycladic Spirit*, without being very well acquainted with the debate which you describe. But it's clearly an issue which presents itself when you have these objects from 2500 B.C. which I feel to be extremely beautiful. The enigma I try to express is, why is it that these works not only strike us as so beautiful, but in some ways look so much like much contemporary art? Isn't that rather a strange thing, that these small



communities of 4500 years ago were producing works which are then set in a gallery purveying twentieth-century art? How come Cycladic art of 4500 years ago qualifies as modern art? There are issues there to be examined.

[Interruption]

SMITH: You were saying that you had something to recount about an experience in Minnesota.

RENFREW: I was invited by the head of the fine arts department to do a semester there, about four years ago, and I just found it a very rewarding experience. It was interesting to be talking in a different way about prehistoric materials, and I had some very lively graduate students. We had a very productive seminar, mainly about group styles, where different students were choosing one culture or sometimes one artist. It was a very interesting enterprise, which allowed one to be thinking more from an aesthetic or at any rate a comparative standpoint rather than about the processes of change and the interaction of subsystems.

SMITH: Were the students working entirely on prehistoric materials?

RENFREW: Not at all. They were in the art history department and so they had no special knowledge of prehistory; that's what made it fun, really.

SMITH: But the subjects that they worked on for the seminar ranged across the time spectrum?

RENFREW: Yes. Some of my lectures were relating to megaliths and themes of



culture change, so those were essentially archaeological. But I did lecture on the work of Richard Long, for instance, and on British . . . one uses the term "landscape art," but that's shorthand for it. We chose different styles, some of them of culture groups, I mean ethnographically, and some ancient styles, but also some more recent ones.

SMITH: With *The Cycladic Spirit*, there is aesthetic interpretation, but you are also a scientist. How did you think the book differed from what an art historian might have written? Did you think about that issue while you were writing it?

RENFREW: I didn't think about that specific issue. I'm not sure how far it would differ from what a classical art historian would have written, except that a classical art historian would have made more of the scholarship—though in fact the footnotes are accurate and do survey the literature quite well enough. And I suppose most classical archaeologists, working in a much more developed discipline, would have perhaps held back from some of the more sweeping, perhaps indeed more naive statements. I suspect a classical archaeologist might say, "Goodness me, these points have been discussed before, this is a rather naive statement." Which in some ways may be true, but one can only write taking note of what one already knows.

SMITH: Now, you have noted that you feel it's incorrect or anachronistic to refer to these materials as art to begin with.

RENFREW: I've come to that view, yes, because if you use the notion that these



objects are art, it tends to lead you into circularities, or into rather pointless discussions such as, what is art? And the question, what is art? is not I think in any way a question about something out there in the world; it's simply asking, what is it exactly that I choose the word "art" to mean? So I think we do much better to escape that and open the way to more relevant questions. We find these objects beautiful. It may not be easy to know, but it would be interesting, at any rate, to speculate about whether those who created them found them beautiful in any comparable way. I know what's been written about aesthetic evaluations of a kind from the time of the ancient Greeks, and I know there is a body of literature, though I'm not familiar with it, about Chinese appreciation of painting and of ceramics. I don't know in what other cultures it can be shown that there was an explicit appreciation of the aesthetic merits of pieces. So it's interesting to ask, to what extent was there such appreciation, and to what extent are the beauties which we feel we perceive in pieces perhaps implicit in the work of those who made them, but not explicitly formulated? Well, those are difficult questions to answer, but at least they seem to me interesting questions rather than saying, well is it art or is it not art? There you are just getting caught up in meaningless verbalism.

The other question, which I think is much more open to examination and hasn't really been examined yet, is, what was being communicated at the time these pieces were made and how does that differ from what is being communicated now?



You have the notion of the message, and the person sending the message, the channel and the receiver. That is usually conceived of in telegraphic terms; you can then maybe switch it round and the person receives in Morse code and then taps an answer back down the line. Well, how does that model for communication break down when the person sending the message is not only dead but dead some time ago? No doubt those questions have been asked and answered, but I'm not aware of the literature on it. Clearly, there is an approach down what might be described as a semiotic avenue of discussion which I think may well be profitable. We may learn more about the pieces, and indeed ultimately about the circumstances which lead us to feel that one piece is beautiful and another piece is not beautiful. I don't think it helps at all to formulate these questions right up front in terms of art and aesthetics.

SMITH: You've mentioned [off-tape] that Duchamp became an intriguing figure for you. I'm wondering which Duchamp is the one that you've been thinking about? Which period and what is it that intrigued you?

RENFREW: Yes. Well, I've looked at *La mariée mise à nue par ses bacheliers mêmes*, and some of the early, more figurative works, and those are all interesting and intriguing, but it's certainly the act of taking a urinal and signing it R. Mutt and putting it in the Armory Show that highlights the question of what it is that we are doing when we exhibit a piece as a work of art. Clearly, museum curators unwittingly do exactly the same thing when they take some very commonplace utensil or hand axe



and put it in a showcase and light it in a special way so somehow it implies that we are supposed to be thinking of it as beautiful. No doubt there are books written about the very subject of what Duchamp was doing, but it is fairly clear, as I understand it, that his action was primarily a polemical or political action, ridiculing the excessive admiration and financial value accorded to some works of art. I've certainly read that that was closer to his intentions. It does seem in retrospect that what he was doing can be seen as something much more interesting. Certainly there's no doubt at all that if that particular urinal could be unearthed in some cellar, signed "R. Mutt," it would at once be worth a million dollars, which would make Duchamp turn in his grave, or enjoy a good laugh. It is the case, is it not, that replicas were produced in the forties or fifties?

SMITH: He produced replicas, yes. And miniatures as well.

RENFREW: That's right, in limited number, and authorized them and they are now hugely valuable.

SMITH: How does your interest in objects that we could call art fit in with your longer term interests about cognitive archaeology and language spread? Is there some unifying element that's underlying these three interests?

RENFREW: Unfortunately I don't see any great relationship with the language-spread idea. It might be thought to have something to do with cognitive issues and perhaps it does, but, in a way, it's something of a diversion from the deeper problems



relating to cognition and the way concepts emerge. I remember when I gave the Huxley Memorial Lecture at the Royal Anthropological Institute on questions of linguistic and genetic diversity, Ernest Gellner was in the chair and gave his comments, and he implied that it was all a bit of a displacement from the real issues about what culture was about. It's perfectly true that if one answers some of these linguistic questions from the archaeological point of view, one is just establishing the history of the evolution of those language families which we see in the world today, which, when all is said and done, tells you very little about the development of human cognitive faculties, which one might suppose to be an ultimate goal of inquiry. So, unfortunately, it doesn't integrate very well.

But the starting point for my interest in the field was, as we were saying, the misconceptions in European prehistory which the Indo-European question raises. Having got into the Indo-European question, it somehow led on to the others. Moreover, it's been worth carrying on, because a lot of people have said we should dismiss this neolithic farming-spread business, but one reason for looking at it on a wider level is that if you find a number of cases where farming dispersals correlate with language areas in a way that feeds back to the Indo-European case. But already this issue is leading us away from the more difficult ultimately and perhaps more interesting cognitive issues.

The other part of your question is, does this fit in very well with some broader



picture? It ought to, and I would like to work more on that. One thing that is very difficult to understand or explain—I imagine that it never really will be explained—is how in some cultures you find particular forms of symbolic expression and in some cultures you don't. It is extraordinary that it is in France and in north Spain that you have the painted caves, and although you do have paleolithic symbolism elsewhere, for instance in South Africa, and in Australia in particular, it's not really the same as the Franco-Cantabrian mural art; it's a very strange thing that that should be so. Very often people base their generalizations about "early man" using these particular examples, without really saying to themselves, "These are exclusively from France and Spain and nowhere else in the world." I'm not saying there was anything necessarily special about those folks in France or Spain, but there was something special about the combination of circumstances that led them to do these extraordinary paintings of animals.

SMITH: I'm sure you are familiar with the people who have been arguing that textiles was probably the most important art form for—

RENFREW: I haven't heard that asserted for the paleolithic period. I could well think that for the neolithic period, when you have evidence of fibers and you have indications of weaving and so on in different areas. Say, a site like Çatalhöyük, where it was James Mellaart who pointed out that some of the mural paintings there may relate to textile motifs. So that is very interesting, I quite agree. It's not beyond



knowledge. For instance, some of the desiccated corpses from the Tarim Depression in Xinjiang date back to 2000 B.C. and have very well preserved textiles, and of course you do get textiles from Egypt as well as Peru, so we do have a number of sources of information about textiles.

SMITH: At the risk of overgeneralizing, we could perhaps say that in the period in which you began studying archaeology, the reigning conception of science was as an orderly pattern of accumulative growth: you would put the bits and pieces together and there might be these punctuations where we suddenly learn a lot more than we had before. In the last thirty years we have had the growth of poststructuralist thinking and the critique of Feyerabend, and the philosophy of science. In the social sciences in particular, we're shuffling interpretations. We're basically constructing meaning that serves the present as opposed to accumulating knowledge. It's not quite that simple because it's an overlapping situation, but are there things that we know now that we didn't know in 1960? And I mean "know" in a hard sense of the word, as opposed to simply narrating different stories about things.

RENFREW: Well, yes, there are. First of all, I'm not sure that among scientists there would be disagreement about the notion of science as accumulating knowledge. That doesn't in any way militate against perhaps the Kuhnian view that within your body of accumulating knowledge you have major fields of theory which undergo transformations that are quite radical; in other words, it's not just adding little bits on.



You do have great areas of revisionism and new constructs which do affect what has been previously understood and make it understood in a different way, but I don't think that militates against the notion of science as increasing a body of knowledge. I would have thought that most practicing scientists would still have that view, and the notion of reshuffling wouldn't apply—mainly, of course, because the data set is always increasing. So that although in cosmology you seem to have tremendous reversals and to-ing and fro-ing, nonetheless, part of what activates and motivates that to-ing and fro-ing are new elements of data which have to be integrated and sometimes therefore cause basic reconfigurations.

I think the same can be said about archaeology. It does differ from some fields in the humanities in that there is the continuous acquisition of a great deal of new data. This may not be true of some periods of history where there is a body of knowledge which is mainly based on the written records. There, to some extent, you are reinterpreting, though no doubt there is more information coming in. But when you ask me if there are things that we know now that we didn't know twenty or thirty years ago, obviously we could point to various fields; for instance, we do have an understanding of chronology which leads on to a whole series of other things, and we have new discoveries. If we are talking about human origins, one could look at the notion of human biological anthropology in Africa, and look simply at the fossil remains that have been discovered. We know about *Australopithecus*, but we still



don't know for sure that *Homo sapiens sapiens* originated only in Africa. That's an ongoing debate, but it's a debate that I don't doubt is capable of resolution; it probably will be resolved in ten years—some think it's resolved already using the molecular biology which is part of the story.

I think perhaps your question also throws up the issue of what things do we feel we know through the development of concepts, for instance, notions of explanation in archaeology and so on. The field of prehistoric trade, which is partly based on the development of techniques of characterization of materials, also has been the subject of a great deal of thought. We know a good deal about early trade and exchange which we didn't know before. That still begs the question of whether that could be reconfigured in such a way that our present knowledge would seem to be only partial or be partly contradicted by further work. But then it's always been recognized as a property of science that it can be corrected and refuted. I'm sure today it would be agreed by pretty well all philosophers of science that science involves reconfiguration—Einstein in relation to Newton would be a perfectly good example.

[Tape XI, Side Two]

RENFREW: I'm very confident that the New Archaeology, in seventy years time, will still be seen as a constructive reconfiguration incorporating new data, as well as new ideas of what happened before, and that will be true for whatever further



transformations occur; these are not just empty reshufflings. They are partly guided by the need to incorporate new data, some of which may contradict what was known before, much of which simply doesn't sit very well with what was known before and therefore determines or requires a reconfiguration.

SMITH: And of course new techniques are emerging all the time, but will we have the funding sources that we've enjoyed the last fifteen years?

RENFREW: Ah, now here's a very different question indeed! We're coming down to a different kind of pragmatics. But there is no doubt that even if it's derisory and minimal, as it may be becoming, there will still be further discoveries, and there will still be some new techniques coming through, so that I don't think that the restriction of funding sources need alter our view of what scientific progress may be. Certainly it will affect the pace of the march of knowledge, yes, but not the nature of how knowledge is changed and whether it's a transformation or a reconfiguration.

When you speak about funding sources, I'm not altogether pessimistic. For one thing, the notion of salvage archaeology is very widely accepted now, and in many countries developments in the city are accompanied by the notion that the developer should pay for an environmental impact statement. But the heritage movement hasn't really linked forces with the Green movement, and it's very strange that it hasn't. They are still rather separate developments, yet they spring from very similar perceptions and I would have thought they would increasingly interact—in the



sense that what is preserved from the past is something to value in the same way as the environment. So this is a potential source of funding. Then there should be funding for pure research; that is certainly much diminished for financial reasons in Britain. In the United States I see where funding for the humanities has been diminished significantly through the bigotry of some political groups there. I'm not sure to what extent that applies to science funding. It's the humanities that have suffered most, is it not, in that direction?

SMITH: Science funding has been cut as well. The arts have had it the worst, humanities next and even science.

RENFREW: But that's an oscillation; one hopes that's not a permanent thing. There are always periods of decline as well as of increase in funding for pure research.

SMITH: It's hard to know and the situation might be complicated because there's no particular reason why the federal government should be the source of funds. There may be other ways of achieving the same results.

RENFREW: Yes. I think there are real grounds for pessimism in some senses.

Certainly in Britain, you know, one laments the day where one could mount the large digs overseas. There was Sir Leonard Woolley, with two or three hundred Arabs all digging away, and four or five European supervisors. It may not be an altogether bad thing that those times have changed. One does now dig more slowly and collects whole classes of material for specialist examination, which means that very small digs

[Faint, illegible text spanning the main body of the page, appearing as horizontal lines.]

in terms of the volume of soil shifted can be very informative. But that very circumstance means that we don't have very many of the big digs now that allow you to expose whole segments of a city at the same time. That is now very rare, not just through lack of funding, but because it's perceived that you are losing vast quantities of data when you do it.

SMITH: So it's a balancing of two different types of strategies?

RENFREW: Yes, that's right. Which is very difficult to do; indeed it relates to what we were saying earlier, that these days archaeologists tend to be much more problem oriented. It may not be exactly hypothesis testing, but it's not very different, and what you find goes way beyond the problems you are interested in. So what do you do about it? It's always one of the dilemmas of archaeological research.

SMITH: But in a project like the Agora project in Athens, it was clear that from the beginning their conception of what they wanted to reconstruct was I think quite different from what you are talking about.

RENFREW: Yes, but at the same time they did act in a responsible way; in other words, if you found something you had to do something about it—conserve it and publish it. So whatever their guiding motivations, they were still also guided by that general archaeological ethic. Which is perhaps more difficult to express in detail now, because there are different ways of digging, but if you find the material you've got to respect it. You've got to publish it.



SMITH: Even if it doesn't fit.

RENFREW: Yes, but worse than that: if it doesn't fit it might still be interesting, but even if it's totally tedious, you still have to respect it. You know, there used to be a time when if people were interested in ancient Greece they'd just throw out the Roman material. "Roman muck" was a term that Dick [R.] Hope Simpson I remember used to use in site surveys. If he was doing a site survey, seeking interesting bronze age sherds, if he found later material he was inclined to say, "Oh, Roman muck," and throw it away. Well, it's increasingly realized that we have a responsibility to conserve these things. Also, there are reasons for looking at things diachronically, right through to the present.

SMITH: What interpretations have you made over the last thirty-five years that now seem to you less solid than before, or on the contrary seem to you now more solid than ever before?

RENFREW: It's a curious circumstance that I've never asked myself before, "Now, where exactly was I wrong?" Perhaps that shows a shocking immodesty of some kind.

SMITH: Well, I purposely chose the formulation "less solid" as opposed to "wrong."

RENFREW: It's true that things I wrote thirty-five years ago, for instance, parts of my doctoral dissertation, though seeking to argue against a paradigm which focuses above all on chronology and culture history and relations between cultures, perhaps



inevitably it's very much using the language and the thinking of that paradigm. In fact, I've been aware that once one has asserted something, it's sometimes a mistake to go on refuting the critics of what you've asserted, because when you assert that something isn't valid, you may well be asserting there are better ways of looking at it, which is certainly true if we're talking about the diffusionist picture or advocating a processual view. Then people say, "No, you've got your chronology wrong, we don't agree with this." If you go on demonstrating to them that you don't think you've got your chronology wrong and they are the ones who are wrong, then you are still using the same language, and you're still fighting in that old paradigm.

When I wrote the book *Before Civilisation*, I laid a great emphasis on chiefdoms. The great merit of the concept of chiefdom was that it allowed you to break from the dichotomy that either everything was very simple and egalitarian or it was a state society and very civilized. It's clear that there are intermediate forms of society, and that some of those in prehistoric Europe could be so described, and that is what we want to focus on. Well, if only I had written a little more in that way rather than relying very heavily on the concept of chiefdom, it wouldn't have opened a way for people to say, "Oh well, we examined this concept of chiefdom, we didn't find it totally useful, so we don't agree with that book." There are quite a number of cases where if one had only said a little more, in a manner that wouldn't have been very difficult at the time, one would have anticipated things that were said ten years



later with great *éclat* as a matter of great revelation. So I have that awareness, but that obviously is very much after-wit, and if thirty-five years later you see that there are some things which you could have said that you didn't say, it's not a cause of huge surprise.

As to what things were actually wrong, I just got copies of a new Italian edition of *Before Civilisation*, which was first published in 1973, and it is almost surprising that there isn't really anything very much in it that one would have to correct. There are plenty of cases where you could say, "Oh well, one could have said a bit more there, why on earth didn't one develop that idea a little further?" For instance, there is all this stuff written by British archaeologists about looking at megalithic tombs and thinking more about what they meant to those who made them, which is part of the so-called postprocessual approach. Well, I certainly developed some of these ideas in television and radio programs, the notion of these monuments as territorial markers in segmentary societies. I can see that it would have been helpful if I had developed a little more the experiential standpoint [in *Before Civilisation*]. Some of those ideas were already clear to me. They are being disseminated now by somebody like Chris Tilley, who has written *The Phenomenology of Landscape*. He has actually been walking up and down cursus monuments and asking, "What did it feel like to be there? What did these monuments mean to the people who made them?" I think that's all perfectly valid, but to me it is



massively unsurprising and could have been said twenty-five years ago. Perhaps I did begin to say it, but I feel rather sad now that I didn't develop that idea.

SMITH: There are aspects of that in *The Cycladic Spirit*.

RENFREW: Yes, but that's much later. *The Cycladic Spirit* was just written a few years ago, and indeed, it isn't really asking, "What did it feel like in those communities?" It's focusing particularly on, "What did it feel like to look at those marble figures?" Which is, perhaps, rather a narrowly restricted frame of reference.

SMITH: But that's not really the nub and the essence of what you mean by cognitive archaeology.

RENFREW: No, it isn't, that's right. I am sure there are developments to come, but it means sitting down and developing various fields through. One insight that I did have, which I wrote about in my inaugural lecture already, was the case of those Indus Valley cubes. (Small stone cubes, found at sites of the Indus civilization such as Harappa and Mohenjodaro, which, when weighed in the laboratory, can be shown to have weights which are multiples of what must be an early unit of weight. Without making assumptions that their modes of conceptualization are analagous to our own, it can be shown that there was the equivalent of a unit of weight used in weighing operations, which carries implications for the measurement of commodities and perhaps their value in exchange terms. This is a case where modern observation of the archaeological record can make positive assertions about earlier thought



processes.)

It has always seemed to me a very good example of refuting those skeptics who say, "Oh you can't say anything about past thought from the artifacts. That's an absurd ambition." Which is what people were saying before the New Archaeology came about. It was one of Binford's complaints, people saying, "Oh, you can't say that from the data." Well, it is impressive how much you can begin to say. If you have what you can plausibly demonstrate or prove is a system of weights, well, that's very interesting; you are really saying quite a lot. I don't think that point in itself, although I've written about it, has been developed very much. It is clear that before you are going to do various things in a well-structured society, you are going to need systems of weights and measure. Interestingly, this is one of the points Dr. McDonald made with his own fascination with this issue.

Edmund Leach wrote about weights and measure from an anthropological point of view, seeing the importance of these things. And yet, just as Jack Goody has written about the significance of writing to early thought, I think there's a whole comparable field about the use of measure, which implies units of measure, in early thought. You can develop techniques by which units of measure may be recognized from the archaeology. It's worth talking about because you can see how to handle this, and that gives you whole possible insights into approaches to the material world of the persons living in that material world. There are whole areas I think that await



further investigation and funnily enough, people aren't considering them very much. I don't really understand why.

SMITH: I don't think we have very much more to talk about at this point, though perhaps down the line, in reviewing the transcript, we may feel that something may need to be added. But since this is in some ways part of a project that relates to art history, perhaps we should think of some of those issues a little bit more, and how they relate to prehistoric art objects or symbolic objects. Actually, I was wondering, is it fair even to say they are symbolic objects?

RENFREW: I would have thought that they are, virtually by definition. Obviously, it would depend on what you meant by "symbolic," but the general simple definition of a symbol is something that stands for or represents something else. If we accept that these are figurations, and Cycladic figures are clearly human forms, it would be difficult to doubt that in that very minimal sense they are symbols—as indeed any pictorial representation is a symbol. There may be other meanings of the word "symbol," where we could have something interesting to talk about, but there they just clearly fall within that definition, I would say.

SMITH: Right, well then, I think we can say they are symbolic objects.

RENFREW: I think we can agree with that probably, yes.

SMITH: Can a social scientist look at these objects as a scientist and not simply as an aesthetic observer? And is it possible to construct a science of art?



RENFREW: It would again depend what you meant by "art." If you simply meant a science of the history of representation, then it wouldn't be surprising if one concluded that one could, because although as we were saying earlier, it may actually in a formal sense be quite difficult to demonstrate that these objects are representations of something else, nonetheless, we would all agree that they represent the human form and there wouldn't be much doubt about it. So, clearly, you can recognize them as representations of the human form, you can discuss what aspects of the human form they emphasize, how their creators set about representing the human form, their history as objects, and what they were used for. All of those I think could quite naturally fall within your rubric of the science of the history of art, if by "art" one simply means visual representation. In earlier days, if you went to see an art exhibition, you would be going to see an exhibition of visual representations. Using the word "representation" in the literal sense of re-presentation, in other words, an image of recognizable reality in a literal figurative sense. But your question was close to being, Can one have a science of aesthetics? Wasn't that really what you were asking?

SMITH: Yes.

RENFREW: Talking about the definition of art as something which arouses or provokes your emotional response. Somebody like John Searle, with his thoughts on consciousness would say, "Yes, consciousness is a subjective experience, but there is



no reason why one shouldn't study subjective experiences." Even though we cannot actually experience another person's subjective experiences, we can still form opinions about them, for instance, by what they say about them. They may not be telling the truth, or they may not describe the experiences well, but nonetheless, that is a source of information which may carry something of interest. So from that rather tediously logical point of view, the answer might be yes, we can have a science of aesthetics, but I'm sure most of us would suspect that there is a different answer.

I imagine when most art historians are talking about the history of art, they are talking about their own responses and their own judgments, rather than about mass observation: what do people say about this or that? They are talking about their judgments and the judgments of others whom they respect. The question, Can there be a science of the history of art? is like the question, Can there be a science of literary appreciation? I can see why the answer might be no. If, ultimately, you are talking, as I think you may be, about one person writing about what they feel and why they feel it, really in that sense I think art criticism is autobiography. Now can there be a science of autobiography?

SMITH: Or can there be an internal science of autobiography?

RENFREW: That's right. Of course, on the other hand, I'm sure it is the case that most art historians have hoped, like many scientists, that by their own experience they will, through their sensitivity, perceive aspects of the world to which they can draw



the attention of others and which are found to command assent. Indeed I think probably this is the case with many successful art historians. They have been able to frame their understanding and appreciation in such a way that it has received agreement; although perhaps in retrospect that agreement has only been temporary.

The only writers on art theory that I have really read very much of and been influenced by are people of the generation of Herbert Read—British art historians or thinkers about art. Herbert Read was one of those who dwelt on the concept of significant form, and his view of abstraction would be very much in harmony with the way somebody like Henry Moore might well have spoken about his own work. Somehow by simplifying, the artist hits on particular forms that are significant and following that approach, there begins to be almost a sort of mysticism, that there may be some particular forms that are just right. Just as there was a sort of mysticism about the golden section, and the notion that maybe there are particular principles of proportionality that get it just right. Of course the ancient Greeks may have thought in those terms and certainly some twentieth-century artists have done so, and some critics.

SMITH: [Constantin] Brancusi, certainly.

RENFREW: Did he actually say that about proportion in that way?

SMITH: Yes.

RENFREW: Certainly, but it was [Amédée] Ozenfant wasn't it who actually thought



there were precise ratios. I think today probably there would be fewer who would feel that somehow if you could just find it, somewhere, there's a special proportionality. I'm not sure that carries so much conviction now.

SMITH: You have mentioned Herbert Read, and of course that then brings us back to Roger Fry, and then back yet again to Ruskin. I wondered to what degree, as you were writing *The Cycladic Spirit*, you were aware that you were in a tradition of British art criticism?

RENFREW: Not strongly at all. I've never really read anything much of Ruskin, though I have of Roger Fry and Herbert Read, so it may well be that some of my preconceptions have been formed by their ideas. I am not very conscious of that, but it seems to me quite likely.

SMITH: If I understand correctly, we can make speculations, but we don't really know if these figures were intended to be pacific in spirit, or frightening, or awe-inspiring—

RENFREW: That's right (although we would know more if they had not been divorced, through looting, from their contexts of use). We simply do not know the full range of their uses. Most of those where we have evidence of their find circumstances, at any rate most of the complete ones, have been found in graves, so it has often been inferred that they were made specifically to accompany the deceased. But because they are quite often broken and repaired, and because you quite often



find fragments of them in settlements, I suggested that it's quite possible that they were used during the lifetime of their owners, that they actually had a use in a settlement context, and then for whatever reasons came to be buried with the deceased. So that obviously gives a completely different suggestion: if they were made to accompany the deceased, then they are funerary figurines; if they were made for use of some kind and then accompanied the deceased, that's something else.

If they were made for use in the settlement, it's possible they were used in connection with religious observances, and that led me on to the suggestion that the almost life-sized ones might conceivably have been used not for domestic religious purposes but perhaps for collective ceremony, in other words for religious ritual, and so there might have been sanctuaries where such things might have been used. By way of comparison, small Mycenaean figurines were found in domestic contexts and large ones were found in what we would regard as shrines, and in Greece today, large icons belong in a church, but there are also small, domestic icons for use in the home. So you can make all that seem plausible, but because of the terrible misfortune that most of the figures come from looting, we do not know the answer. Not one of the big figures has been found in a really satisfactory archaeological context.

So we don't know, but you can certainly imagine that the large figures may have been effigies that were used in the context of cult. That in itself doesn't tell us a great deal about their meaning, but it does then lead you to the possibility that they



may have represented a deity or deities. Which would not of course be excluded in the thought that they were made specially for funeral use, but it might be a different kind of deity. I think one is entitled to embark on speculation, which is to some extent structured by the data. You can find supporting elements there, but unfortunately not more than that. Many of these figures had been found on Keros, in fragmentary condition. When we excavated there I had hoped for some further insights, but the excavation didn't lead to any obvious new information concerning context. We didn't find a little corner of a shrine with an effigy, as we did at Phylakopi in the late bronze age. So, you are right, if we were embarking on an inquiry of how the Cycladic figures were used, and in that sense a step towards what they meant to those who made them, we still don't know, despite a century of archaeological investigation. The main reason we don't know is because of the looting of course.

SMITH: I had one last element of questioning, which has to do with the nature of the archaeological community, and I use the term "community" instead of "profession" in this case because it strikes me that it is a field that brings together both the academic and the amateur.

RENFREW: That's certainly true—and a wider population which is interested sometimes in a very constructive way.

SMITH: People who have nonacademic professional interests, such as salvage



archaeologists, but perhaps that's more the case in the U.S. than in Britain—

RENFREW: No, it's so in this country, but they often are professionals.

Numerically, the bulk of professional archaeologists are now salvage archaeologists.

SMITH: Within this broad range of people you have antiquarian types, diffusionists, processualists, postprocessualists. Does the community, such as it is, function in a constructive, interactive way? Do you have forums where you can sit down with Ian Hodder and share views which may be conflicting but nonetheless may get you both to think about something in a different way?

RENFREW: Certainly. I'll start with the atmosphere in Cambridge, since Ian Hodder and I are both members of the same department. It was the case twenty years ago that quite a lot of the Cambridge prehistorians were on bad terms with each other. Grahame Clark was no great friend of Glyn Daniel and vice versa; I'm not sure Glyn Daniel was a great pal of Professor McBurney, and Eric Higgs was skeptical of many things outside the study of bones and environmental approaches. David Clarke was something else again; he was of course a research student, then a junior archaeologist, but he wrote rather scathingly about some more traditional archaeologists of an older generation, including Glyn Daniel. So the department at that time was not one where people would sit down in that way. (That being said, however, the coffee room of the faculty had an important role in those days—not least for facilitating communication between archaeologists and anthropologists, who meet together more rarely now.)



More recently, all the members of the department are on perfectly cordial terms, and there are occasions in the department when there'll be a number of us in a seminar exchanging views. The truth is that Ian Hodder and I took part in seminars much more frequently when so-called postprocessual archaeology was rather newer, about ten years ago, and there was a good deal to argue about. Indeed there still is, but in a way we've had many of those arguments and we've both moved on a little in the sense that much of the strong polemic between processual and so-called postprocessual archaeology has been argued through. Indeed, so-called postprocessual archaeology has rather dissolved now into a whole series of archaeologies, so that Ian Hodder would write of "interpretive archaeologies" in the plural, and they indeed often are in disagreement with each other. But for that reason it's often the case that we tend to meet together and exchange views at larger conferences and meetings. I mentioned earlier the Theoretical Archaeology Group, but there are others.

I think the British archaeological community, particularly the theoretical archaeologists, are very willing to sit and talk with each other. But while we are willing to talk about some quite severe or quite radical theoretical differences, it should be remembered that many traditional archaeologists are traditional not because they have a very clear idea of archaeological theory, but they are entirely atheoretical, or would wish to be considered atheoretical, and among these are many digging



archaeologists; this is true both of academics and of some salvage archaeologists. So there is a big division there in the archaeological profession now, particularly with the rise of salvage archaeology. This situation is more severe in the United States than in Britain. The salvage archaeologists are busy people, they've got to go out there and dig this material up. Sometimes they publish it if they get around to it, but in the United States often they don't have time; this is not true of all of them, but it's true of some. They don't have time for archaeological theory. They say, "Excuse me, I've got digging to do." They really have got mud on their boots, as it were.

If you go to the annual meeting of the Society for American Archaeology, which used to be, as I implied earlier, the great forum for theoretical discussions, now it's largely about what the local archaeological officer should do next, and problems of funding rescue archaeology and so on. In this country the Institute of Field Archaeologists has an annual conference, and that tends to address itself rather more towards those problems, and of course the Theoretical Archaeology Group still largely is theoretical. In a way, this was true also when the New Archaeology was introduced. It wasn't always New Archaeologists arguing with other academic archaeologists. Kent Flannery related some very amusing narratives comparing the graduate student with the "real Mesoamerican archaeologist," the guy with the trowel in his pocket who would do the digging.

So there are different divisions. Archaeology in Cambridge, for instance, has



the reputation, especially among field archaeologists in Britain, of being a bunch of people talking theory all the time; they never really get on and do the digging. It's not altogether fair, because most of us have done excavations and many continue to do so. But there's a certain feeling: Let those theorists get on having their arguments in their ivory tower and meanwhile we'll get on with the real stratigraphic archaeology. There are other ways of delineating community differences, and of course there have been strong arguments and dissents, but I don't think recently we've had the vituperative severity of argumentation which there sometimes was in the past. I don't think we waste too much time just losing our tempers with each other at the moment.

SMITH: Do the field archaeologists have something constructive to provide to the theoretical archeologists?

RENFREW: Well, they of course have the results of their excavations, which include new discoveries, sometimes of significance. I suppose it's fair to say that some of the improvements in archaeological recording methods, such as on-site use of computers, have come from university archaeologists going into the field and trying to be innovatory from some sort of intellectual standpoint, but other innovations have come from professional diggers just being efficient—a lot of the people in the field archaeology unit at English Heritage are of that kind. They have brought about interesting improvements in field recording procedures, but they haven't yet I think fed through to the point where archaeologists undertaking synthesis use records



which are in a computerized form.

It is true that one of the problems of archaeology is the vast masses of data, which are not always drawn on very effectively. There are great problems associated with publishing excavations. There is a publications backlog, and there is the question of whether the excavation archives, the whole body of data, have been adequately prepared. Many problems with the archaeological enterprise, I believe, are problems of a practical nature, and I'm not sure that the theoretical archaeologists if you can call them that, do involve themselves as much as perhaps they ought to with that kind of archaeology.

SMITH: Well, if data is theory-laden, then whether one knows one has a theory or not, when you go out into the field, what you find in some ways is related to what you expect to find.

RENFREW: That's right, but in the archaeological world you will have many people who pride themselves on being atheoretical, who, if pressed, will admit that they operate on a basis of common sense. Well, of course "common sense" is just an unformulated theoretical position, really. It would be an interesting assignment for a graduate student to go around to these people who say they have no theoretical position and establish what that position is by a series of well-judged questions. One could do that. Their theoretical position would in the main I think be the accepted position of thirty years ago, updated by some technological advances. So that if we're



talking about field archaeologists who perhaps have a theoretical position of thirty years ago, that's in no way suggesting they're not up to date very thoroughly with appropriate investigative techniques.

SMITH: So then part of the challenge to the theoretical archaeologist is to convey the theory in some meaningful way so that field archaeologists would say, "Yes, this is an interesting set of questions."

RENFREW: I think that's absolutely right, and I suppose, although one doesn't always set oneself a sort of pedagogic program of that kind, the fact that one publishes one's work does set a framework for discussion. The book *Archaeology: Theories, Methods, and Practice*, which I wrote with Paul Bahn, in some ways may well go some direction towards that objective, because Paul and I have been quite astonished at the success of the book in terms of reviews—I've never written anything that's been so favorably reviewed—and in terms of being adopted for university courses very widely around the world. So it must be then that the view of archaeology inherent in that work is being taken on board very widely.

SMITH: Well, I think I've run my course for the time being. I offer the opportunity if you have anything more you would like to say.

RENFREW: I believe I have been offered quite adequate opportunity up to now, though once we survey the six-hundred page transcript, maybe lacunae will become apparent.

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